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AN EXPERIMENTAL INVESTIGATION OF THE POSITIVE AFTER-IMAGE IN AUDITION[1]

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Our problem is to find out if there is a positive auditory after-image, analogous to the positive after-image in vision, and in that case to describe it in attributive terms. If we detect the presence of auditory experience after the stimulus ceases, we must seek to identify this experience as after-image, or memory after-image, or memory image, or whatever it may be.

[1] From the Psychological Laboratory of Cornell University.

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Historical

The first experiments upon the after-effect of auditory stimulation appear to have been made by Mayer.[2] The sources of tone were tuning-forks supplemented by resonators. The tones were conducted to the one ear by means of a rubber tube; the other ear was plugged with wax. Between the nipple of the resonator and the free end of the conduction-tube stood a siren disk. When a space between the holes in the disk was before the nipple, the sound waves were blocked; when a hole was before the nipple, the sound passed into the tube at full intensity. As the disk was rotated, short periods of stimulation alternated with equal periods without stimulation. The critical value sought was the length of the interval between tones which would just be bridged, without loss of sensible intensity, by the after-effect, so that the *O* should hear a smooth tone.

Urbantschitsch[3] worked by a similar method. The tones were interrupted by a pendulum, which carried the one end of a conduction-tube to and fro past the limbs of a Y-tube. The tube to *O*'s ear, which completed the conduction system, was connected to the stem of the Y-tube. Unlike Mayer, Urbantschitsch sought to determine the critical interval at which the tones would just fall apart. The aim of the experiments was to measure the full duration of the after-effect, whereas Mayer measured only that part of it which showed no decrease of sensible intensity.

Schaefer[4] criticizes the method of interruptions on the ground that it cannot take separate account of the *Abklingen* and *Anklingen* of the tones employed. Marbe[5] also considers the method inadequate. "An investigation of the facts of *Abklingen* in auditory sensation can not be made . . . by means of successive, periodic stimuli, but only by the more difficult means of isolated auditory stimuli."

Urbantschitsch[6] further studied what he calls "primary" and "secondary" positive auditory after-images. The "primary" after-image is so closely joined to the sensation that there is no perceptible break between the two; the "secondary" after-image follows the sensation only after an interval, and may recur several times, at intervals of varying length. Urbantschitsch's method is so imperfectly reported that it is impossible to undertake a repetition of his experiments. We are told only that three tuning-forks were sounded at four intensities (characterized as "very strong," "strong," "moderately strong," and "weak") for periods of 5 and 15 sec.

Experimental

We employed two sources of tone: Stern variators, with tube-transmission, and vibrating telephone receivers. It may be said at once that the principal difficulty with either source

[2] A. M. Mayer, "Researches in Acoustics," *Amer. Jour. of Science and Arts*, 147, 1894, 1 ff.

[3] V. Urbantschitsch, "Ueber das An- und Abklingen acustischer Empfindungen," *Archiv. für d. ges. Physiol.*, 25, 1881, 323 ff.

[4] K. L. Schaefer, Nagel's *Handb. der Physiol.*, 3, 1905, 507.

[5] K. Marbe, "Akustische Prüfung der Thatsachen des Talbotschen Gesetzes," *Archiv für d. ges. Physiol.*, 100, 1903, 557.

[6] V. Urbantschitsch, *op. cit.*; and "Zur Lehre von der Schallempfindung," *Archiv für d. ges. Physiol.*, 24, 1881, 585 ff.

was to secure a clean cut-off of the stimulus-tone. Our successive arrangements, therefore, represent repeated efforts at improvement in this respct. The experiments were performed in a suite of three rooms. It was necessary to spread the apparatus over a wide space on account of the power of tones to penetrate even heavy stone walls. Accordingly the *O*'s sat in one room, *E* operated the apparatus in another, and a large unoccupied room lay between. Even so we found that the sources of the higher pitches at greatest intensity must be enclosed in soundproof boxes.

Series 1

In our first series of experiments the source of tone was the variator. Four pitches were used, 1024, 512, 256, and 128 vs. The variators were blown at three intensities, and for three durations of stimulation. The three intensities were obtained by setting the air-pressure at 15.0, 7.6, and 4.0 mm. of water for the pitch of 1024 vs; at 5.0, 2.6, and 1.6 mm. for 512; at 19.0, 11.6, and 8.0 mm. for 256; and at 8.0, 5.5, and 4.0 mm. for 128. The nozzle of the variator was so adjusted to the mouth of the cylinder that a maximum of intensity should require the lowest possible pressure. We found that the pressures could not be equated, but varied from instrument to instrument, as indicated by the pressure-values just listed. The durations were 5, 15, and 30 sec. read directly from a Pye clock. Since the seconds-hand of this instrument moves over a dial of 11 cm. diameter, the spaces between seconds are broad enough to admit of accurate reading.

A special control enabled us to adjust the air-pressure quickly and accurately. A wooden lever 85 cm. long, screwed to the handle of the rotary valve, swung before a ruled pressure-scale. From one side of the lever and 60 cm. from the valve, two metal arms (2 cm. long and 0.5 cm. broad) extended parallel to each other and at right angles to the edge of the lever. They were 4 cm. apart, and a fine wire was drawn taut from the outer end of the one to the outer end of the other. This wire, parallel to the edge of the lever and to the lines of the scale, lay on the surface of the scale in such wise that the error of parallax was avoided.

The sound-waves were picked up by a funnel of cardboard and reflected into a brass conduction-tube of 1.9 cm. inside diam. and 0.15 cm. thickness. The small end of the funnel fitted snugly over the end of the brass tube; the diameter of the large end was 15.0 cm.; and the length of an element in its surface was 45.0 cm. This slant is that of an old model of Victrola horn, and is approximately the same as the slope of the funnel in Marbe's *Sprachmelodie-Apparat*. When the variator was set in position, its mouth was in line with the axis of the funnel, just not touching the edge.

The brass conduction-tube was 8.76 m. long. It passed through two walls, across the middle room of the suite, and ended in *O*'s room in an iron pipe 2.3 m. long. The iron pipe, which lay horizontally against the wall and at right angles to the brass tube, served as distributor of the tones to the four booths in which the *O*'s sat. Four nipples were tapped into it, and from these four rubber tubes, of 0.6 cm. inside diam., led to four pairs of stethoscopic ear-pieces. The brass tube

entered the iron pipe at its middle, and the nipples were at 36 cm. and 104 cm. right and left. Under these conditions, the intensity at all listening tubes was sensibly the same. The booths in which the *O*'s sat were separated by heavy curtains.

We have described above the valve which controlled the blowing pressure. It was necessary to introduce another valve between the pressure-valve and variator, which should cut off the air suddenly and bring the tone to a sharp ending. The special valve constructed for this purpose somewhat resembled the piston-valve of a cornet. In one position of the piston, the air went straight through the one opening to the variator, while the other opening was closed at the tube in which the piston moved. In the other position, the passage was reversed; the hole which had been open was now closed, and conversely. In this second position, no more air could enter the part of the tube between valve and variator, and the air which was present under full blowing pressure when the valve first closed had two outlets, the one by way of the nozzle of the variator, the other by way of the passage through the valve. Since the latter passage offered the lower resistance, most of the pressure was spent in this direction, and the terminal "whoop" which otherwise was very troublesome to the *O*'s was reduced to the vanishing point. (Unless the air escaping from the tube, in the process of reduction of the pressure within it to atmospheric pressure, can be diverted from its course through the nozzle, the pitch of the tone in dying falls quite perceptibly as the pressure diminishes.) The diam. of the holes through the piston, 0.6 cm., was the same as the inside diam. of the rubber tube carrying the air to the valve; so that the piston need be moved no more than 0.7 cm. to open the one passage and close the other. In the first series of experiments we threw the valve by hand.

As warning signals we used four small 4-volt incandescent lamps placed upon the wall before the *O*'s. The circuits were so arranged that the light could be made to glow at two intensities. The lamps were turned on at the lower intensity at the beginning of the experimental hour, and remained at this intensity throughout, except when they were flashed as signals; at the lower intensity they showed merely a dull red glow. Our object was, by keeping the filament constantly warmed, to increase the sensitivity of the lamp. We needed to have it flash brightly in an instant, an impossible result if the filament were cold at the outset. *E* closed a key to flash the usual "Ready," "Now" signals. When the air was turned on at the valve, the same movement closed a mercury-contact key attached to the piston of the piston-valve, and the lights burned brightly. When the air was cut off, the movement of the piston broke the circuit, and the lights dropped to the lower intensity. This method secured temporal coincidence of the cutting-off of the tone and the dimming of the lamp.

Every *O* had at his hand a key making or breaking the circuit in the recording apparatus. This apparatus consisted of eosin writers adapted to use with ticker paper. They were made of tin; and though different in structure their principle of operation was that of the draughtsman's ruling pen. They wrote from the horizontal position, withstood hard knocks, and did not spill the fluid when tapped rapidly against the paper. When *O*'s key was closed, the tip of the writer was pulled against the paper by an electromagnet, and a line was written representing the length of any after-effect which might be experienced. When *O* was ready for the next tone he tapped the key as a signal to *E*. The writers were so slender that we were able to

write one time-line and four record-lines upon the usual ticker paper 1.8 cm. in width.

The dashes in the time-line were made by a Kronecker interrupter vibrating in tenths of sec. To fix the exact moment at which the tone began, we resorted to another mercury-contact key upon the piston of the piston-valve. When the air was cut off, the circuit was closed, and the time-writer wrote a continuous line; when the piston moved to turn on the air, the circuit was broken, and under power from another circuit the interrupter-point began to write tenths of a sec. The paper was drawn by a motor which ran throughout the experimental hour; but the feed of the paper was controlled by a clutch, and it was drawn past the writer only when needed. With this arrangement there was no lag of the paper; it was drawn immediately at full speed.

Observers. The *O*'s were Mr. R. T. Holland, graduate student and assistant in the department; Miss M. F. Martin, Dr. C. W. Perky, Miss A. H. Sullivan, and Mr. S. Takaki, graduate students majoring in Psychology. Observer P was somewhat more highly practised than the others, but all were experienced *O*'s.

Instructions. "You will hear a tone which will begin when the signal light brightens, and will cease abruptly as the light dims. You are to note whether the tonal sensation continues or recurs after the dimming of the light. If it continues, you are to press the key immediately and release it at its final disappearance. If it recurs, you are to press the key at every appearance and hold it until the disappearance of the recurring sensation. When the experiment is over, you will write a descriptive account of the after-sensation or after-sensations on the blanks provided."

For every pitch there were 9 stimulations presented in the order: strong-short; middle intensity-middle duration; weak-long; strong-middle duration; strong-long; middle intensity-short; middle intensity-long; weak-short; weak-middle duration. This series of 9 stimulations was run off first with the highest pitch, then with the remaining three pitches in descending order. There were thus 36 experiments in a series. Every series was repeated five times, giving a grand total of 180 experiments for every *O* with this first arrangement of apparatus.

Results

Our results showed a considerable number of cases in which the tones "ended abruptly," were "chopped off," with no trace of terminal modification or after-effect. Besides these, however, there were cases described in which the ending of the tone was modified, in quality, in intensity, or in temporal

course. The *O*'s spoke of "slow dying," "prolonged ending," "not abrupt," a "whoop." Never did the modified ending carry sensibly beyond the dimming of the light, or suggest to *O* the continuation of the sensation in a positive after-image.

Table I is a summary of all modified endings. Unfortunately, we cannot be sure of their interpretation. The slow cooling of the filament in the lamp may have led to error in judgment on the part of the *O*'s. The apparatus could not guarantee that what was heard was the natural ending of a suddenly stopped tone; so that these modified endings may

TABLE I

0	Intensity			Duration			Pitch				Total
	Strong	Medium	Weak	30	15	5	1024	512	256	128	
H	22	18	7	17	14	16	18	14	14	1	47
M	30	27	29	29	32	25	28	20	20	18	86
P	36	29	22	27	30	30	24	23	27	13	87
S	37	32	38	39	38	30	35	34	16	20	107
T	30	25	20	26	26	23	22	19	23	11	75
Total	155	131	116	138	140	124	129	110	100	63	402

in fact have followed the last vibration, and thus may represent an after-effect. Besides this serious defect in the apparatus, our first series of experiments indicated that several minor changes were necessary. Noises in *E*'s room reached the *O*'s through funnel and conduction tube; the purr of the motor which drew the ticker paper had in it a note that was easily mistaken for an after-image; the hand-operated valve was not always thrown with equal speed from the one position to the other; and any echo of the tones in *E*'s room might reach the *O*'s.

Experimental: Series II

We chose the lights as signals because they were silent; but they were not reliable; and we now substituted for them small electromagnets from a player-piano. These magnets are not noisy; and when operated by a weak current, just strong enough to produce a light, quick movement, they did not annoy the *O*'s. To the armature of the magnet we soldered a strip of tin, 4 cm. by 0.5 cm., to serve as signal flag. Every *O* had a magnet and flag in his booth before him, at the place where the light had been. When the current was off, the flag stood almost vertically; when it was turned on, the flag was pulled quickly down to the horizontal.

In order to operate the piston-valve quickly and at a constant speed at all times, we built two solenoids end to end on a single base. They were mounted at the end of the piston, and an extension of the piston passed through them both. One solenoid pulled the piston to turn the air on, the other pulled it back again to turn the air off. With a sufficient current the shift was almost instantaneous.

We shut out the noises from *E*'s room by placing the variators in a sound-proof box. We cut the funnel down to a smaller size (diam. 10 cm., length 25 cm.) for use inside the box. The whole conduction system was now isolated, though there was still the possibility of echo from the walls of the box. Indeed, we found that there was a faint tone which made its appearance regularly at a short interval after the air was cut off. We satisfied ourselves that this tone was in the apparatus, and was not subjective, by making records of the vibrations. A diaphragm of the Marbe *Sprachmelodie-Apparat* was held firmly against *O*'s end of the conduction tube, and the smoke-rings registered the tone.

To rid ourselves of this source of error, we so hinged one of the sections of brass conduction-tube that the one end of it could be drawn aside. The other end was attached by a heavy rubber tube to the fixed end of the next section of conduction-tube. An electromagnet in series with the solenoid which cut off the air to the variator pulled this swinging section of tubing to one side at the same time that the variator ceased to be blown. In this way the echo was diverted into the middle room of our suite, and the *O*'s could not hear it. We found it very difficult to swing the pipe noiselessly, but we finally hit upon the following arrangement. One end of a thin strip of wood, about 1 m. long, was fastened to the floor with a hinge; the other end carried the swinging end of the conduction-tube. The electromagnet was fastened to a firm support about 30 cm. from the floor. A piece of iron, screwed to the thin strip at this level, was attracted by the magnet, and the pipe was thus drawn aside. The iron was faced with felt; and by this means, supplemented by the elasticity of the strip of wood, the pipe was brought to rest silently in its new position. The supporting wooden strip stood so nearly vertical that it was easily pulled toward the magnet; but it was inclined so far away from the vertical that gravity pulled it back to the first position when the circuit to the magnet was broken. A piece of spring steel, faced with felt where it touched the wooden strip, furnished a silent stop for the pipe when it returned to the position for conduction. The swinging section was 1.82 m. long; with a radius so large, and an outside diam. of the tube of only 2.2 cm., the space between the swinging end and the fixed end of the next section could be about 0.25 mm. The space was adjusted at every experimental hour so that the ends should just not touch. The break in the pipe was 84 cm. from the point at which the brass tube joined the iron pipe in *O*'s room. We felt satisfied that, with a distance so short as this, no residual sound-waves remaining in the conduction system between the *O*'s and the break in the tube could have any effect for audition.

In the light of our first results, it is clear that the warning signal must be very accurately timed. The modification in the ending of the tone was so short that the flag must move in exact coincidence with the last sound-wave. We turned again to the Marbe apparatus. It was set up with two diaphragms and one gas-burner, all in the same gas system. One diaphragm made the tone-rings, the other the ring from the flag. The diaphragm at the flag was brought so near the flag that the first beginning of movement was recorded by the membrane. By this arrangement the effects from the two membranes were superimposed. The record should show a band of regular rings representing the tone, and then an aperiodic ring written by the flag, followed by a band with no rings. By carefully adjusting the contact placed where the swinging section of conduction-tube would close the

circuit to the flags, we were able to secure this kind of record. The heavy ring from the flag showed as the last ring in the series, and it stood at the proper distance from the last ring but one to be in coincidence with the last tone-ring. To be sure, the record did not indicate whether the heavy ring was the result of a summation of two rings; but it did show that the flag was not more than one vibration out of step with the tone (256 vs.). Still, however, there was the possibility that the stroke of the flag blotted out the tone-rings; tone might be present, but unrecorded. We put this possibility to the test by allowing the flag to make its ring without cutting off the tone. We found that the tone waves were just as distinct in the record after the stroke of the flag as they were before. The disturbance in the flame set up by the flag did not blot out the effect of the tone, but gave a superposed effect.

With these improvements in the apparatus we were ready for the next set of experiments.

Observers. The *O*'s were H, M, S, T of the previous group.

Instructions. "You will hear a tone which will begin when the signal flag falls, and will continue for a varying period of time. About a second before the tone ends, the flag will fall as a warning signal that the tone is about to end. Just at the instant that the tone ceases abruptly, the flag will fall again. You are to note whether the tonal sensation continues or recurs after the flag falls. If it continues, you are to press the key immediately and release it at the final disappearance of the after-sensation. If it reappears, you are to press the key at every appearance and hold it until the disappearance of the recurring sensation. When the experiment is over, write a descriptive account of the after-sensation or after-sensations on the blanks provided."

The experiments in this series were conducted with the same tones as before, in the same order. Except for the changes in the apparatus, already described, and for the warning signal given during the course of the tone, the method is the same as in the first set of experiments.

Results

The distribution of the modified endings is shown in Table II. The decrease in number between Series I and Series II

TABLE II

0	Intensity			Duration			Pitch				Total
	Strong	Medium	Weak	30	15	5	1024	512	256	128	
H	5	1	0	2	3	1	2	0	4	0	6
S	35	28	26	36	32	21	32	30	18	9	89
T	6	2	2	7	3	0	4	2	0	4	10
M		(none)			(none)			(none)			
Total	46	31	28	45	38	22	38	32	22	13	105

seemed to indicate that further refinements might lead to their complete elimination. We, therefore made additional efforts to refine our technique.

Experimental: Series III

So long as the tones were stopped by cutting off the air, there was escape of the air under pressure between the nozzle of the variator and the piston-valve. An unknown part of this air must certainly escape by way of the nozzle; and its escape, if audible, might lead to *O*'s reports of modified endings.

Only after we had discovered how difficult it is to avoid the influence of the diminishing air pressure, did we turn to the rather obvious plan of allowing the variator to blow continuously, while we broke the path of conduction in order to obtain the periods of silence between stimulations. We accomplished this by swinging more sections of the conduction-tube. The section at the sound-proof box was pivoted at the middle to allow the ends to move up and down. The fulcrum was a wad of cotton, packed rather tightly around the pipe in the hole in the stone wall through which it passed. The next section was joined to this lever of conduction-tube and to the third section in the line by rubber tubing fitted over the ends of the pipe. When the end of the pipe in *E*'s room was thrown down, the end in the middle room went up, carrying with it the end of the second section. This movement produced a break in the line at the box, but preserved the continuity in the next room. At the other side of the room was the section adjusted to swing in the previous set of experiments. The swinging end was now supported by a wire and pulley, so that it could be drawn up and dropped down. The means of obtaining simultaneous and equal movement at both breaks in the conduction system was very simple. The wire just mentioned extended across the middle room; its ends dropped vertically down from the pulleys and were fastened to the conduction-tube, the one end to the end of the lever section, the other to the free end of the swinging section. When the end of the lever at the variators went down, the end in the next room went up by an equal amount, since the fulcrum was in the middle. This upward movement allowed the wire to roll over the pulleys, and the end across the room dropped by an amount equal to the movement of either end of the lever section. The displacement thus secured was about 5 cm. The pull upon the pipe was exerted by means of a solenoid fastened to the floor in *E*'s room. A cord tied to the end of the lever-section supported the core at a good pulling distance above the center of the solenoid. With a strong current, the displacement was sudden and free from noise. We found it necessary, however, to sew the core of the solenoid into a close fitting bag, in order to secure silent action of the core within the coil.

It is obvious that the shape of the ends of the conduction-tube at the break could not be circular. The diminishing amount of area for conduction as the ends became separated might be responsible for an observable decrease in intensity of the tone. The ends of the tube, except for a slit of 1.8 cm. by 0.3 cm., were accordingly plugged with sealing wax. The long dimension of the slits was horizontal, so that a quick movement of the end of the pipe downward brought them wide apart, too rapidly for any sensible decrease in intensity. We tried at first to use four of these slits, but found that they reduced

the intensity; two at the sound-proof box were sufficient. The circuit driving the signal flag was again closed by the swinging pipe.

We found that the two high tones could be heard three rooms away, even though the variators were in a box with very heavy double walls, if they were sounded at more than our intermediate intensity. We accordingly decreased the blowing pressure to 2, 4, 7.6 cm. of water for the tone of 1024 vs.; and to 0.9, 1.6, and 2.6 cm. of water for that of 512 vs. The two breaks in the conduction-tube weakened the low variator tones beyond usable limits. In their stead we used tuning-forks (with resonators) of 256 and 128 vs. The resistance in the circuit was such that at 10 v. we obtained satisfactory driving power for the forks at 1, 2, and 3 amps. for 256 vs.; and at 2, 3, and 5 amps. for 128 vs.

Results

The results from this series of experiments are shown in Table III. We reserve comment upon them until later.

TABLE III

	Intensity			Duration			Pitch				Total
0	Strong	Medium	Weak	30	15	5	1024	512	256	128	
S	30	26	20	25	27	24	30	33	7	6	76
T	1	3	4	3	3	2	3	3	1	1	8
Total	31	29	24	28	30	26	33	36	8	7	84

Experimental: Series IV

The apparatus just described was satisfactory except for the fact that the tones were weak. As it stood we could be reasonably certain that the *O*'s heard the natural ending of a tone, the sensible process of transition from tone to silence. The noise incident upon swinging the pipes was, it is true, barely noticeable just after the ending of the tone was heard, and a very faint after-image might have been lost. But the main fault lay with the intensity of the stimuli themselves. We required a source which should have fairly intense tones, and which should be completely silent in the period following the cessation. After a good deal of cut-and-try experimenting, we had recourse to telephone receivers.

The instrument was the ordinary variety of receiver of 75 ohms resistance. It was actuated by a Petzold double induction coil, with a current of 2 amp. at 6 v. through the two primaries connected in series. The vibration frequency in the receivers was determined by the number of interruptions in the primary, and not by a separate source of sound. By this method we avoided the complications which a transmitter would have introduced. The interrupters were tuning-forks of 435, 350, 256, and 100 vs. We were unable to drive the forks satisfactorily by the same current which passed through the primaries of the coil; by means of an extra pair of contacts put upon the forks, we passed two separate currents through them, the one driving the fork, the other leading to the induction coil. A condenser connected across the point of interruption in the circuit to the coil kept down the sparks at the interrupter, and gave a tone free from the usual click and buzz of receivers.

The line across which the receivers were connected derived its power from the secondary coils connected in series. A special switch with tion. After a good deal of cut-and-try experimenting, we had re-breaking both sides of the line at once. When one side alone was broken, the receivers sounded faintly; and they could be made silent only by interrupting both sides of the line together. This same switch also carried contacts controlling the signal flags. When the path to the receivers was broken, the circuit to the flags was made, just far enough in advance to counteract the slight lag in the flags.

The three intensities of stimulation were obtained by setting the secondary coils both at 0; the one at 14, the other at 2; and both at 14. There was no further change in the experimental procedure.

Results

Table IV contains a summary of modified endings from Series IV of 360 observations for every *O*.

TABLE IV

	Intensity			Duration			Pitch				Total
O	Strong	Medium	Weak	30	15	5	1024	512	256	128	
H	120	120	0	80	80	80	60	60	60	60	240
S	119	120	113	120	118	114	90	89	87	86	352
T	6	2	1	2	3	4	1	0	4	4	9
Total	245	242	114	202	201	198	151	149	151	150	601

Significance of Results

So far as concerns a positive after-image, analogous to the positive after-image in vision, we have nothing to report. In a total of 3,780 observations, the results have been flatly negative. But our 'modified' endings are positive results; and as they have been ineradicable, we are obliged to discuss them.

The instructions laid emphasis upon the positive after-effects of our stimuli; and the different *O*'s took different attitudes toward the endings of the tones. In Series I and IV, H reported the modified endings frequently; in Series II, he was especially attentive to after-effects, and only the more pronounced of the modified endings were reported; in Series III, he made the general statement that he could hear a "ylup," if he made an effort to hear it, coincident with the drop of the flag. Throughout the series he took the instructions literally, and reported "Nothing" for the after-effect of every stimulus. M never reported modified endings after Series I, and like H never heard an after-effect. T reported modified endings very infrequently after Series I, but he never totally ignored them. S failed always to hear after-effects, but she persistently gave her attention to modified endings.

An examination of the totals in Tables I to IV shows that for every series the frequency of the report of modified endings is directly proportional to the degree of intensity of stimulus. Even in Series III, where the strongest stimulus was rather weak, the rule holds in some measure. The influence of duration is less regular; but there are always fewer modified endings reported at 5 sec. than at 15 and 30 sec. In the case of pitch, the tables may be misleading. In Series I and II, decrease in frequency of modified endings parallels a fall in pitch; but the high tones were intense and the low were weak; so that what might be taken for an effect of pitch is in fact an effect of intensity. We feel assured, from the results in Series III and IV, that this interpretation is correct. In both these series all pitches were of nearly equal intensity, and the distribution of modified endings is also nearly equal for all pitches. The contradiction in Table III is only apparent. It was very difficult to keep the tuning-forks of 256 and 128 vs. vibrating at low voltages, and for this reason we gave only one series with each. At higher voltages the forks had so great an amplitude of vibration that they struck the core of the electromagnet; for this reason it was impossible to grade the intensities from a higher maximum.

Throughout the earlier series, we regarded the modified ending as due to a fault in technique. We believed that refinement of apparatus would remove it. The agreement between number of modified endings and intensity of stimulus supported this opinion; but the influence of duration was against it. In order to explain the effect of duration, we searched the results to find what terms were used in describing the modified ending. Many references to it were not descriptions, but characterizations like "swoop," "whoop," "sigh," "groan," etc. In Series III, H and S agreed that the difference between the tone and its ending was, in part, one of 'vocality,' which changed as the tone went off. S further described the ending as having "less body than the tone;" it "disappeared out in space," "decreased in volume," "thinned out," "lost quality," etc. We have set down the attributive conditions of stimulus under which such descriptive terms were used in Table V. The results are all taken from Series III, since it is in this series only that the *O*'s maintained a constantly descriptive attitude.

The uniform distribution for duration shows that the modified ending is not dependent upon differences in that attribute of stimulus.

TABLE V

	Intensity			
O	Strong		Medium	Weak
H	120		120	0
S	37		41	43
	Duration			
O	30		15	5
H	80		80	80
S	40		41	40
	Pitch			
O	435	350	256	100
H	60	60	60	60
S	33	42	26	20

In the same group of experiments, certain terms indicative of judgments of intensity, like "pronounced swoop" and "less ŭh," were used. Their distribution is shown in Table VI.

TABLE VI

	Intensity		
O	Strong	Medium	Weak
H	120	120	0
S	10	5	3
	Duration		
O	30	15	5
H	80	80	80
S	6	6	6
	Pitch		
O	435	350	256	100
H	60	60	60	60
S	10	2	1	5

The extreme regularity of H's results is due to a 'set' for intensity. He reported only upon the relative intensity of the modified endings, which he found to be well above the limen. The figures show again that the modified endings are independent of duration. In Series I, where S frequently gave judgments of length of modified ending, the same result was found: the modified ending was short for all durations of stimulus.

It will be remembered that in every series every degree of intensity was given once with every duration of stimulus. In Series III, we counted the number of times that S used the same term to describe the modified ending for all three durations in a series or for all three intensities. The number of cases is shown in Table VII.

TABLE VII

INTENSITY

Strong	Medium	Weak	Total
12	8	1	21

DURATION

30	15	5	Total
1	2	2	5

PITCH

435	350	256	100
9	6	2	9

It is evident that duration does not markedly affect the modified ending, seeing that for 12 of the 15 times that the strong stimulus was given *O* is able to call the effect by the same name in spite of the 3 different durations of stimulus. The single case recorded in column 3 is very misleading in that 7 of the 12 weak stimuli were negative, *i.e.*, ended abruptly for sensation, leaving only 5 times that a single term could be used. The table shows that duration had a slight influence in determining the descriptive term; but it is only about one quarter as effective as intensity. The 7 negative cases were distributed, 5 to the short, and 2 to the intermediate duration.

In Tables I to IV, difference of duration appears to be effective in determining the number of reports of modified ending, and may, therefore, be a factor in determining their existence or non-existence. In Tables V to VII, duration does not modify the character of modified endings. Our experiments offered the suggestion that pressure in the ears[7] may have been of importance in forcing attention to the modified ending. T reported pressure or deafness following the stimulus under conditions which are shown in Table VIII.

[7] On the perception of silence, see E. B. Titchener, "A Further Word on Black," *Jour. of Philos. Psychol. and Scient. Methods*, 13, 1916, 649 ff.

These results are all taken from Series IV, in which the stimuli were sufficiently strong to make pressure and deafness moderately conspicuous. We conclude from them that both intensity and duration co-operate to bring about a state called sometimes deafness, sometimes pressure. The only time that a weak stimulus produced pressure was when its duration was 30 sec. Here, then, is an effect of duration which may be responsible for its otherwise unexplained influence upon the frequency of report of modified endings. Through it, only those modified endings will be reported which force themselves to the focus of attention. Our instructions, however, tended to direct attention to the period following the ending of the tone. Hence we should expect a report of pronounced or conspicuous endings, but of no others.

TABLE VIII

INTENSITY

Strong	Medium	Weak	
19	18	1	Pressure
4	7	0	Deafness
23	25	1	Total

DURATION

30	15	5	
18	12	8	Pressure
6	2	3	Deafness
24	14	11	Total

PITCH

435	350	256	100	
19	4	9	6	Pressure
4	3	4	0	Deafness
23	7	13	6	Total

In order to substantiate this explanation, it becomes necessary to show, by a special direction of attention, that modified endings are, in reality, always present. H had already said that he could hear them with every stimulus, if he tried. Prof. H. P. Weld (W), and Dr. L. B. Hoisington (Ho.), members of the department and highly experienced *O*'s, had previously been called in for supplementary observations following Series III. At first, they reported modified endings, but when they assumed a passive attitude toward them, and directed their attention to the silence following the end of the tone, the number of modified endings observed became fewer. Their reports indicated, nevertheless, a possibility that all endings of tone are modified.

To put the matter to the test, we made a few experiments, with Dr. K. M. Dallenbach, instructor in the department, H, Ho, M, T, and B (the writer) as *O*'s.

Experimental: Series V

The stem of the Y-tube at the ear-pieces was held in the left hand; and the end of the rubber tube, which previously had been joined to the Y-tube, was held in the right. Both free ends were brought together, just not touching, for stimulation, and were suddenly drawn apart to remove the stimulus tone. The movement was made in all possible directions, with equal effect upon the auditory experience; and the thumb held against the end of the rubber tube to cut off the tone had no different result from that noted with movement of the tube. These movements provided a soundless means of discontinuing stimulation. *O* was asked, first, to direct his attention to the change in the character of the tone just as it ended, and to report whether it could always be heard; secondly, to turn his attention to the period of silence following the tone, and to report, from stimulation to stimulation, whether the silence seemed more or less distinct from time to time; and thirdly, to report the times when the modified ending especially attracted attention. Stimulations were given for 5, 15, and 30 sec.

Results

Every *O* was able to hear a modified ending of every stimulation, if attention were directed upon it, even with weak tones carried almost to the limen. Every *O* found that the stimulus of 5 sec. was incapable of producing any but the faintest pressure which is characteristic of perceiving silence. Durations of 15 and 30 sec. definitely produced the pressure; but the *O*'s did not agree whether 30 or 15 sec. produced the greater pressure. Every *O* found also that the modified ending was not insistent after the 5 sec. stimulation, but that it stood out very conspicuously from stimulations of 15 and 30 sec. In B's experience the heaviness of the deeper silence from long stimulation worked, as if by contrast, to make the modified endings stand out more vividly without necessarily becoming more intense. It seemed to be the 'contrast' between silence and modified ending which grew more intense as the stimulus-time was lengthened. Since the pressure is cumulative with the product of intensity and duration of stimulus; and since the intensity of the modified ending seems to depend upon intensity of stimulus; we conclude that the enhanced 'contrast' effect comes by way of increased pressure.

This, then, seems to be the explanation of the apparently contradictory results. The modified ending varies in intensity directly with intensity of stimulus, and is always present. When the *O*'s report abrupt endings, or fail to report the end-

ing at all, their direction of attention to the period of silence is complete; and no endings, except the most vivid, come to their notice. Long stimulations enhance the contrast-effect between tone and silence, and increase the number of times that *O* hears the modified ending; they thus give the impression that the longer the stimulus-time, the more frequent is the modified ending.

Nature of the Modified Ending

It is tempting to regard the modified ending as 'subjective' in nature, and to identify it with the *Abklingen*[8] or 'drop' of auditory sensation which Mayer and others have attempted to measure. This view is supported by the facts that the modified ending varies directly with intensity of stimulus, and that it shows always a thinning-out or reduction of the 'body' of the tonal complex. While, however, we have no wish to dispute the presence of an *Abklingen*, we are nevertheless of the opinion that, in our experiments, this subjective factor was outweighed by objective changes.

In the first place, we mistrust the results from the telephones. So long as the tone is stopped by taking away the driving power of the diaphragm, there is a source of error remaining: the diaphragm must come to rest from the position in which it happens to be when the current is cut off, and this return is definitely audible. Our arrangement with induction coil and condenser served to eliminate the noisy click natural to the instrument at the moment when it ceases to be under the influence of the electromagnet; but it is evident that the very fact of vibration still necessitates a return to a state of equilibrium.

For a similar reason, the results from the variators are not free from objection. When the variator ceases to blow, we have no means of knowing through what stages the physical stimulus passes in coming to rest. At one moment the air is in motion, and a moment later it comes to rest. Our modified endings may depend in part upon objective change in the sound waves. When the path of conduction is broken by pulling the conduction-tube aside, eddy-curents must be set up around the ends of the tubes; and these, though not audible at low intensities, may change the character of the stronger tones as they end.

[8] For *Abklingen* in general see C. Stumpf, *Tonpsychologie*, 1883-90, references in index: K. L. Schaefer, Nagel's *Handb. der Physiol.*, iii., 1905, 504 ff.

There are, indeed, positive indications that the modified ending is in part objective. (1) The results from Series I-III differ widely with respect to the number of times the pitch changed in any direction. A summary is given in Table IX.

TABLE IX

	Series		
	I	II	III
Pitch rises	6	25	48
" the same	48	15	23
" falls	11	7	5
Total	65	47	76

In Series I, the vibration in the air spent itself normally. In Series II, when the pipe was drawn aside, the path of conduction was interrupted; and eddy-currents at the break may have changed the character of the ending of the tone. The ends of the tube were not plugged; and even though the pipe moved quickly, a certain brief time was required for the movement. In Series III, the ends were plugged, and the movement of the pipe was quicker than in Series II. The eddy-currents would be more intense, owing to the greater speed with which the pipe moved and to the narrowed opening in the pipe. Table IX shows that the increase in the number of judgments of higher pitch, and the decrease in the number of lower pitch, are correlated with the suddenness of the ending of the tone and the violence of the movement of the pipe. (2) We made a few experiments as a check upon the implication of Table IX by the method of pulling the rubber tube away from the stem of the Y-tube at the ear-pieces. When the end of the rubber tube vibrates rapidly before the end of the Y-tube, the movement produces a distinct thud or noisy puff. If the movement be made sufficiently violent, the noise covers up the tone. B, H, Ho, M, and T all reported that the modified ending was most intense, at any intensity of stimulus, when the rubber tube vibrated 4 to 6 times per sec. This result proves that long stimulation does not add to the intensity of the modified ending, but that rate of vibration does, probably from the increase in energy of the stimulus due to interference of air-waves at the ends of the tubes. (3) B and Ho made a number of observations to determine the pitch of the modified ending, and found that it is dependent upon the rate of movement of the end of the tube. A quick movement gives an "oop" higher in pitch than the tone; and a slower movement gives the same kind of sound, but its pitch is lower, and it is softer and has greater volume. Certain

rates of movement give a pitch that is equal to that of the generator. Both forks and variators were used as stimuli. If these observations, made under loose experimental conditions with hand-control of the speed of movement, may be trusted, it is possible that we have an explanation of the results in Table IX; for the judgment of pitch of the modified ending is correlated with the objective suddenness of the movement which interrupted the tones. The observations also furnish additional evidence that the modified ending may be objective;[9] and that, if a means of interrupting tones could be found which did not affect the air in the path of conduction, it might in so far be eliminated. It may, however, be impossible to deprive a tone of its normal means of propagation, the air, without at the same time setting up disturbances in the air.

Such results as ours call for further experiments conducted under the best possible conditions. As our problem was to demonstrate the presence or absence of an auditory positive after-image, the modified endings are incidental to our purpose; and it is unnecessary that we should make a more complete investigation of the conditions upon which they depend. Since, however, their occurence, in so far as it represents an objective disturbance of stimulus at cessation, might give ground for the objection that a possible faint after-image has been covered up and overborne, and thus has escaped the notice of our *O*'s, we have made some further experiments with tones of greater intensity and longer duration.

Experimental: Complex Tones

In order to extend our results to other kinds of stimuli and to long stimulation-times, we conducted a group of experiments with more complex tones. In some experiments the original stimulation-times were used, and in others the time was extended to 5 and 15 min. The tones were C' and C'' from organ pipes of metal; C and C' from organ pipes of wood; and tones number 2, 4, 6, 8, and 10 from an Appunn reedbox of the overtones of a fundamental of 64 vs.

Our *O*'s in this group were Miss E. C. Comstock (C), graduate student in psychology; Mr. F. L. Dimmick (D), assistant in the department; H, Ho, M, S, T; Mrs. A. K. Whitchurch (Wh), graduate student in psychology; and a class of 7 students in the advanced laboratory course.

[9] It was noticeable from the very first that *O*'s who were observing in the same group did not by any means necessarily report in the same way upon the same stimuli. We have already shown that they varied in their attitude, attending sometimes to the tone's ending and sometimes to the period immediately following. The objective or partially objective character of the modified ending is, therefore, not put in question by the variety of report. Moreover, as we have also shown above, prescribed direction of attention led to uniformity of report.

All tones were blown at full intensity, at the different durations. The *O*'s sat where they could see the *E*'s hand move as the tone was cut off. This movement replaced the signal flag of previous experiments. In all, 448 observations were made. Of these 11 were with 15 min., and 7 with 5 min. stimulation.

Results

Not an after-image was reported; but our practised *O*'s heard modified endings, and felt pressure in the ear from long stimulation.

Our regular experiments had indicated that pressure in the ears made its appearance in place of an after-image, and we wanted striking evidence that this indication was true. If naïve *O*'s could be brought to sense the pressure, without practice, this result would be good evidence of its fundamental nature. Accordingly, our unpractised *O*'s were instructed to describe any after-effect of stimulation that might appear. They were then given number 2 on the reedbox as stimulus for 15 min. All of the group, except one who misunderstood the instruction, reported vivid, throbbing pressure which lasted from 5 to 10 min. after stimulation. Nothing was heard. One *O* remarked that there was no after-image, unless the throbbing were a negative after-image; and added that in this condition the ears "felt very sensitive to noise." If 15 min. of stimulation resulted in so perceptible a pressure, it may be supposed that the naïve *O*'s who were able to report it were also capable of perceiving any tonal after-effect, and that their failure to perceive tone is further evidence that no auditory after-image exists. The case of heightened sensitivity to noise may help to explain why, in the regular series of experiments, long stimuli led to more frequent report of modified endings.

Auditory Recurrent Images

For T, the tone frequently recurred after stimulation. S reported only a single recurrence, the other *O*'s none. The attributive correlates of T's recurrences are given in Table X.

The recurrences were generally 1 to 2 sec. in length, but sometimes were as long as 10 sec. More than a single recurrence was not reported after the sources of tone had been placed in sound-proof boxes. There were numerous 'recurrences,' as many as seven, when the purr of the motor could be heard through the conduction-tube. These recurrences are, of course, not trustworthy. As a rule, the recurrent tone had the same pitch as the stimulus; but it was sometimes one or two octaves above or below. Localization was generally

in the head, though sometimes recurrences were unlocalized. B found that, after serving as *E* for one or two hours, the recurrent tone came as an addition to any faint continuous objective sound, but that in a quiet room no recurrent image could be heard. Our recurrent images obviously resemble the "secondary" after-sensations of Urbantschitsch.

TABLE X

	Intensity			Duration			Pitch			
Series	Str.	Med.	Wk.	30	15	5	1024	512	256	128
I	10	11	3	8	9	7	1	8	11	4
II	0	1	1	2	0	0	0	1	0	1
III	8	3	0	3	5	3	5	3	0	3
							435	350	256	100
IV	5	1	1	4	2	1	2	1	4	0
Total	23	16	5	17	16	11				

In Tables III and IV, T's results are often not in agreement with those of the other *O*'s; but it will now be seen that the disagreement is due to the number of recurrences at higher and middle intensities, or in long and middle durations. The unequal distribution for pitch in Table X may be due, in part, to the influence of the register of T's voice; the recurrences are most frequent within his singing range.

Conclusions

1. There is no positive after-image of tone, analogous to the positive after-image of vision.

2. The 'modified ending' of tones, which we have been unable to eliminate, is probably a compound effect, due in part to tonal *Abklingen,* in larger part to the objective conditions of our experimental arrangement.

3. Intensity of modified ending is dependent upon intensity of stimulus; its vividness or insistence upon pressure in the ear, which increases with intensity and duration of stimulation.

4. It is impossible from our experience to say which of our two sources of tone, the variators with air conduction or telephone receivers, is to be preferred. The variator tones were relatively weak; when they are cut off, slight noise accompanies the swinging of the conduction-tube; if the conduction-tube remains unbroken, echo is a source of error. They furnish, however, an almost purely tonal *Abklingen.* The telephone receivers give tones approximately equal in intensity at all pitches; in the period when the after-image is expected to appear, they are absolutely noiseless; there is no difficulty from echo; but the *Abklingen* is very impure, owing to an admixture of noise from the diaphragm.

THE INFLUENCE OF COLOR UPON MENTAL AND MOTOR EFFICIENCY*

By Sidney L. Pressey

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I. Problem

The problem of the study may be put as follows: Do different hues and brightnesses of general illumination have (*aside from* the obvious importance of these factors in conditioning the effective functioning of the visual apparatus) any specific influence upon mental or motor efficiency? The subject is obviously of great practical importance. If adequate lighting in factory or office tends to stimulate activity and increase the amount of work done, as well as to aid in visual efficiency, the matter is of no slight moment from a business point of view. If, as is often supposed, an office or

* From the Psychological Laboratory of Harvard University. The paper is a summary of the writer's thesis for the doctorate, which was presented in April, 1917, but which, because of pressure of other work, it has not been possible until now (October, 1919) to condense and arrange for publication.

school-room furnished in red has a distinctive effect, upon mood and upon sustained capacity for work, as compared with a room furnished in light green or buff, then the definite demonstration of that fact would be of decided practical value. The aim of the present study was to investigate experimentally, in the laboratory, this question as to the effect of brightness and of hue upon mental and physical work.

II. Previous Work Bearing Upon the Subject

As thus conceived, the study differed, in certain important particulars, from other investigations in this general field. In the first place, color combinations were not considered. In the second place, large surfaces, involving the subject's entire field of vision, were used, not small stimulus-areas (usually the subject worked in a room illuminated solely by a light of the desired brightness or hue). In the third place, the interest was in objective measurements of mental work and neuro-muscular tone, not in introspective reports. Finally, as has already been mentioned, the study was not concerned with the effects of hue or brightness upon visual acuity or ocular fatigue; any such effects were carefully avoided. Previous studies in the aesthetics of color have, however, most important bearings upon the writer's problem. A consideration of these previous studies may be conveniently taken up under three heads, according as the work was (*a*) introspective or observational, (*b*) experimental, or (*c*) analytical and critical.

(*a*) *Introspective and Observational Studies.* The earlier work treats rather of the artistic values of the different colors than of the narrower problem of the relation of color to affection. It is impossible in brief space to summarize the mass of this material. It may be said, shortly, that introspective consideration of the matter seemed to indicate that each color conditioned a specific emotional and affective response of great richness.

Goethe's "Farbenlehre" is the most important early treatment. There are "active" and "passive" colors. The hues from yellow through red are exciting, lively, arousing, the climax being reached in the red. From blue back to red is the minus or passive side; and at green is a balance of arousing and depressing influence which gives a feeling of peace and comfort. There is thus a complex and intimate relation between color and affection.[6] Such theories find their best known and most systematic formulation in Wundt.[40]

Anecdotal data with regard to color,[38] anthropological observations,[15] and studies in the history of art,[16] may also

contribute to the problem. Savages, and also many animals, show a marked fondness for bright colors and for shiny, brilliant objects. As evidences of the same tendency in civilized man may be cited the fondness for diamonds and other gems, for fire-works, for lacquer, gilding, boot-blacking, and the adoption of shiny metal for money.[15] Among the hues red has an outstanding position; the power of red to attract attention and arouse action, in both men and animals, is made much of.[10] However, there may be striking differences in the affective character of a hue, from one locality to another or one period to another. Thus yellow was in classical times a favorite color, and is now least liked.[11]

(*b*) *Experimental Studies.* Midway between such observational studies and the laboratory research comes a mass of more or less roughly controlled investigations, mostly with children, of which the monographs of Miss Shinn,[34] Mrs. Moore,[26] Preyer,[29] and Major,[24] and the papers of Winch,[39] Wells,[37] Jastrow,[18] may be mentioned. The gist of the results may be put in a sentence. There is an early fondness for bright colors, decreasing with age; an especially rapid dropping back of yellow, ending in least liking; in general blue and red are liked best.

The laboratory work may be summarized quite as briefly. There is an almost absurd difference of opinion among many of the experimenters as to the affective value of the various hues. And experimental studies of special problems in the aesthetics of color show little more (relevant to the present problem) than the extreme complexity of many reactions to color, and the multitude of factors which may play a part.

Cohn[7] states that his is the first experimental study of the aesthetics of color. He placed his subjects in a dark chamber, and used for stimuli small gelatine plates; the illumination was by daylight. He found, briefly, that saturated colors were preferred, but that as between different hues of equal saturation the choice was a matter of individual idiosyncrasy. On the whole, yellow was least liked; but the data are not considered adequate for any generalization in the matter. Cohn had seven subjects, and worked by the method of paired comparisons. Major[25] used colored papers, and a different method. He had four subjects. Neither antipathy for yellow nor preference for saturation was discovered. Cohn[8] thereupon experimented further, and confirmed his previous results as to saturation. Miss Baker[2] found the warm end of the spectrum most pleasant. Fernberger[13] found relative dislike for yellow and yellow-green. Miss Washburn[36] found that thirty-five college girls liked tints best, shades next, and saturated colors least. Blue was preferred, of the hues.

Some few papers have appeared which try to deal experimentally with the apperceptive elements in color attitudes. Thus Bullough[4] worked on the question of the apparent heaviness of colors. He concluded that a color looks light or heavy according to its luminosity,

stressing this element rather than any association of dark and low objects, or analogous explanations. Pierce[28] found that bright colors "weigh" more than dark ones in making up an ornamental balance; he explains this on the basis of the "action theory" and greater demand of the bright objects for eye-movements. Quantz[20] and Larguier des Bancels[20] agree that color affects the apparent size of objects. Thus red surfaces look larger than equal blue ones. But no satisfactory explanation is offered.

Investigation by the method of expression has led to similar contradictory results. Or rather, it may be said, that the best work of this type has given negative findings.

First to be mentioned of such studies should be Féré,[12] who, while experimenting with hysterics, found the most remarkable differential reactions to different tones, different tastes, different smells, and finally to different colors. If the patient was shown a red light there was an increase in pulse, a disturbance of breathing, a distinct increase in muscular strength as shown by the dynamometer. And, since he considered his neurotics to exihibit in marked degree what was true in kind of normal individuals, Féré concluded that, with normal individuals also, red had a dynamogenic effect.* A Leipzig study by Stefanescu-Goanga[35] comes to a somewhat similar conclusion. Striking results, giving confirmation of the Wundtian theory of the correlation of bodily changes and tri-dimensional feeling, were obtained. However, McDougall[28] found no specific effect of either hue or brightness upon reaction time.† Angell and Thompson[1] found no evidence of any specific association between organic changes and various visual stimuli. Shepherd's very careful work also failed to yield any evidence of such a connection.[33]

(*c*) *Analytical and Critical Studies.* The great importance of social influences, of literary and religious symbolism, and of language, in developing affective reactions to color is the one point constantly stressed by the more critical writers. That other than intrinsic values are important is apparent from the way color preferences change in art, and even with famous artists during their lifetimes. The effect of a color may be totally changed by slight change of circumstances. And such affective toning, though the result of previous associations, need by no means show any trace of those associations. The ideas and feelings have grown together, amalga-

* Féré's work is extremely interesting. But surely these hysterics, who present as perhaps the fundamental symptom an extreme suggestibility, are exactly the persons who would respond most completely to the many subtle suggestions of language, art, and custom, which will shortly be mentioned (not to speak of possible unconscious suggestion from Féré himself). For the distinguishing of a true dynamogenesis, as distinct from the effect of such influences, no subjects could be less reliable.

† Prof. E. B. Holt has told the writer of a very similar study, made by Holt, with completely negative results. The average times, with the different hues, were almost perfectly identical.

mated their effects, until these are indistinguishable from innate hereditary attitudes.

So Müller-Freienfels[27] emphasizes the extent to which a color may be given an affective value simply by its name, as blood-red, or violet, or lilac, or orange, or lavender. The red-furnished room mentioned by Miss Calinich[6] is also of some interest. If the room was warm the reds seemed bright, lively, cheerful, warm and enlivening. But if the room was cold these same reds were dreary, depressing, with a peculiar, dead, chill effect.

The odd artistic career of yellow may also be instanced to show the extent to which even very special and artificial circumstances may have a profound effect upon the esteem with which a color is regarded. Yellow was a favorite color in classical times, and is now in the East. But there is, as has already been mentioned, some evidence to indicate that for the average European it is the color least liked. The change is (according to Havelock Ellis) due to the Church; the early church authorities looked upon the color with disfavor because of its association with pagan festivals and licentiousness, and succeeded in building up about the hue a group of symbolisms and associations of the most unpleasant character.[11]

Previous work bearing upon the writer's problem may, then, be briefly summarized as follows. (*a*) Introspective and observational studies show colors, as they appear in everyday experience, to have a marvelous richness and complexity of affective significance. There is the suggestion that color may be a conditioning factor of distinct importance in influencing the efficiency of mental work. (*b*) Laboratory experimentation for the most part fails to find any constant relationship between color and either introspective evaluation or organic reaction. (*c*) Analytical and critical studies emphasize the importance of artistic conventions, symbolism, language, and everyday association of certain colors to other sensations affectively toned, in giving an emotional connotation to the hues and brightnesses.

It would then seem reasonable to conclude that if color *does* have any fundamental physiological effect, such as would influence mental and motor efficiency, the connection must be of a very general and elementary nature; brightness may stimulate, or red irritate and distract, but more specific effects are hardly to be expected. It is some such relationship as this, between illumination and mental work, which is sought in the writer's experimentation.

III. Experimental Results

A. *Preliminary Experimental Definition of the Problem*

This experimentation began with a brief preliminary investigation, of an exploratory character, in which large sheets of colored paper were used. Nothing more need be said about

the method than that the subject was seated comfortably at a table, with a large sheet of colored paper arranged in front of him so as to be almost the only object within his field of vision. While in this situation, thus dominated visually by the color, the subject was asked to introspect, and to do certain tests. Nothing more was done with the tests than to make trial of them. But the introspection yielded some points of interest. In the first place, the finish and texture of the paper appeared exceedingly important, especially so far as pleasantness and hold on the attention were concerned; the "energizing" or "stimulating" effect seemed more dependent on the hue. Where there is a shimmer, or where the light falls across a rough finish so as to emphasize the roughnesses, the result is peculiarly disagreeable. Any smudges on the paper were sure to be noticed, and were likely to play a part in the association; thus the dominant feeling of one subject for an orange was uneasiness centering in the kinaesthesis of a desire to reach out and smooth an unevenness in the paper. In the second place, the introspection suggested that the special affective values often assigned to such colors as lavender, purple, orange might be due to other causes than the hue itself. Such colors are unusual, and so attract and hold the attention; because they are unusual, what associations they have are definite and concrete, and give great vividness. With repetition, however, these factors usually drop out, and the color is ranked along with the primaries, as more or less bright, stimulating, depressing; is sometimes liked merely because it has in it more or less yellow or blue. Finally, individual differences, both in the evaluation of the different colors and in the general type of reaction to them, were marked. In fact all of the types mentioned by Bullough[6] and Bradford,[8] with variations, appeared at one time or another, from crude sensory effect to complex personalizing attitude. The reports made excellent evidence for the complexity of responses to color mentioned in the preceding section.

Thus one subject reported of a striking reddish purple that he "liked the color. But there was no special effect, aside from the pleasantness; otherwise it was neutral." And at the end of the color period it was "about as before. No great fatigue. The affective value was somewhat less, but fairly durable." Very different are the descriptions of another man, who says of a rough-finished yellow that it is "heavy, like an overcoat; queer feeling; not pleasant, not unpleasant. Impression that color is deep, and thick. Kinaesthetic sensation in finger tips as result of thickness. Heaviness seems referred largely to texture." A light yellow green is "very pleasant, a buoyant feeling." Another day this same subject finds the same color "depressing; the color of grass when it's dying."

More explicitly associative is a report on yellow. "A slight greenish effect; brought up idea of grapefruit tree at the Exposition and emotions going with that trip. Now wonders how got green; had seen stripes of green." An orange was liked; but then, with a thought of the war, came an idea of this as the color of bloody water, and a feeling of repugnance.

Different still in type of response is the characterization of a red as "too aggressive; it pushes itself upon you, won't let you alone, rude. I don't like it."

Enough has already been said to suggest the unsatisfactoriness of colored papers as stimulus areas. In the first place, no adequate control of brightness was possible; but such control was clearly essential if an adequate analysis of the problem was to be made. In the second place, the texture of the papers, and any wrinkles or spots, played too important a part in the subject's reaction to make adequate control possible. And in the third place it was not feasible to arrange the experimental setting, with colored papers, as was necessary for the problem. The problem was to determine the effect of color, as a dominant but unobtrusive and natural element in a situation, on mental and physiological processes. If paper were to be used little less than to have the walls covered with it would suffice. Under the circumstances, trial was naturally made of colored lights.

With regard to the preliminary work with colored lights, nothing more need be said than that it was of the same general character as the systematic experimentation to be described shortly. Work with the colored lights was begun in March (1915) and the remainder of that school year was spent simply in trial of various tests, light screens, and so on. The results to be described in the next section were, therefore, obtained with methods which had been carefully elaborated in this trial series.

The preliminary work served chiefly to develop methods and setting and to make trial of tests. The introspective material may be summarized as follows. (1) Affective reaction to colors is highly variable, both from individual to individual, and from week to week with the same individual. No hue, except perhaps red, may be said to show characteristics of any constancy. (2) The reaction is very easily modified, or often wholly changed, by (*a*) subjective factors such as general condition, mood, constellation of ideas, or chance association, and by (*b*) incidental objective factors such as unevennesses in the colored surface, and especially the texture of this surface and the way in which the light is reflected from it (softly, or with a shimmer, glint, or glare). (3)

The reaction may be of all degrees of complexity, from simple sensory effect to elaborate associative development or subtle personalizing attitude.

Introspective treatment of the subject will be returned to, briefly, later.

B. *General Experimental Setting and Method*

The experimental setting developed during the last part of the preliminary work was continued, essentially unchanged, throughout the remainder of the study. It may be described as follows. One end of a long narrow dark room was shut off by a heavy black curtain, making a smaller room about 9x6 ft. Backed up against the curtain was a table three by five. Behind this, that is, facing the curtain and with his back to the end wall of the room, sat the subject. The table was covered with heavy unglazed drawing paper. Over the center of the table, and 26 in. above its surface, was the light-box (8 by 10 by 12 in.), held by a frame-work from the ceiling. This contained four 100 c. p. Mazdas. In the base of the box were slots, where the colored plates could be slipped in. The light box was dead black outside, but lined with metal to reflect the light downward; on the side of the box next to the subject was a black pasteboard drop hanging just low enough so that none of the direct light came into his face. At one end of the table, and dropping down almost to its surface, was the cord, with its glove finger, for the tapping test. Well off to the other side was the small block of wood with the two keys, for the reaction test; this was hooded over with a piece of the white drawing paper, to make it as unobtrusive as possible. The glove finger and the reaction key block were the only pieces of apparatus visible to the subject.

As has been said, none of the direct light came into the subject's face. The light shone directly, however, on almost the whole surface of the table, but very little beyond this. The situation was thus not at all uncomfortable for the subject. He sat easily at the table; the light came from a position not uncommon in ordinary lighting arrangements. There was no shimmer, glint, or glare, from the unglazed paper, only a soft, diffused, indirect illumination. And aside from this there was only the hardly-to-be-seen blackness of walls and curtain. This was the situation when the test lights were burning; when the "normal" light (a 16 c. p. Mazda fastened to the light-box on the side away from the subject) was on, between tests, the situation was very little changed. The lighting was still indirect, came from the same position approximately, and was of roughly the same brightness as the colors used.

On the other side of the curtain, and against the wall, was the experimenter's table. On this were the various recording devices, the theatre dimmer for equating the brightnesses of the colored lights, the time keys, and so on. Here also was the double-throw switch, arranged to throw on the test lights at the same instant that the "normal" was thrown off (and the reverse), so that there should be no period of darkness, or irregular interval, between the two stimuli. The experimenter's table was lit by a four candle-power lamp in a hooded "goose-neck" holder; it was wholly invisible to the subject behind his heavy curtain.

From now on the study consisted of two separate pieces of experimentation, making use, however, of the same tests, the same methods and the same setting; the "hue series" was planned to discover any possible effects of hue, independent of brightness, and the "brightness series" to isolate any influence of brightness, upon mental work. The plates used in the hue series were complementary blue, pure green, red. The stimulus lights were equated for brightness (by episcotister) in terms of the darkest, blue; to do this a theatre dimmer was used to shift the illuminating power of the lamps according to the absorption of the differenf plates.* The illumination, on the top of the table under the lamps, was about seven candle-meters. The brightness series with which most work was done was also a three-unit series. The dim light was obtained by stepping down the current with the dimmer and putting in the white screen; it gave an illumination on the surface of the table of about one candle-meter. The medium light was given by a single 32 c. p. Mazda, placed in among the larger lamps in the box so as to give the same distribution of light, and thrown in by double-throw switch after the "normal" in the same way. The bright light finally settled upon was the full power of the four 100 c. p. Mazdas.

For one semester, however, a five-hue and a four-brightness series were run. The five-hue series consisted of the hues above mentioned *plus* yellow and white. For white a special plate, a double thickness of "pot-opal" glass, was made; at the above-mentioned brightness the light through this was found to be almost pure physiological white. The four-brightness series consisted of the same two extremes as were used in the three-brightness series, but with the white of the hue series, and a medium bright light made by the white plate with the full power of the four Mazdas behind it, in place of the 32 c. p. "medium."

Three possible criticisms of this choice of stimuli remain for brief discussion. In the first place, it may be said that an effort should have been made to obtain monochromatic colors for the hue series. To

* The writer is indebted to Dr. L. T. Troland for the equating of the brightnesses, and for the rough analysis of the lights here given.

this it may be replied shortly that the problem of the study was essentially a problem in applied psychology, and that what was sought was the effect of colors such as might be used in actual schemes of illumination or decoration. The important requirement was that the lights should be seen by the subjects as red, green, and blue, of a not unusual composition or saturation, and that the saturations should be approximately equal. The lights were, as a matter of fact, almost invariably judged by the subject as simply blue, green, or red without admixture of other hues; and they were considered about equally saturated (the yellow was dropped partly because less saturated). Nothing more seemed essential. Monochromatic light, supposing it could have been obtained for use over such areas, would have been a highly artificial form of stimulus much less relevant to the practical problem.

It may be objected, in the second place, that more hues (and more brightnesses) should have been used. The difficulty was mainly a matter of method, and of experimental practicability. Most of the subjects came to the experiment only once a week, for a 50 min. period; five or six colors meant only seven or eight minutes with each. The problem, however, was to determine, not the effect of the presentation of the color, but the influence of its continued presence as part of a work situation. Longer periods were thus clearly necessary. So it seemed best, for this and various other reasons, to limit the experimentation to intensive study of three hues, and three brightnesses. Practically all the previous work (not to mention the writer's own preliminary study) indicated the colors at the two ends of the spectrum as the hues most diverse, fundamentally, in their effects. It seemed reasonable to suppose, therefore, that an intensive study of red, of green, and of blue would indicate with fair adequacy the direction and amount of the influence, if any were to be found, and give the best evidence for negative conclusions in case the results were negative.

In the third place, criticism might be made of the "very bright" light of the brightness series, as too strong, possibly so bright as to trouble the eyes, and so introduce factors not relevant to the problem. The writer expected such difficulties. And he was much surprised to find, after extended trial with all the subjects, that in most instances this bright light was the most pleasant of the series! The subject received only indirect, diffused light; there was no glint, glare, or other unpleasant distribution. Under these circumstances this light was usually introspected upon as stimulating, cheering, very pleasant. And anything less bright failed to give the desired distinctness from the "medium" light.

As has already been intimated, most of the work was done with three hues, or three brightnesses, to the hour. This permitted 12 to 15 min. with each color, with intermissions between colors of about 4 min. The colors were given in a definite routine; if, for instance, subject "A" was given on the first day red first, green next and blue last, then for his next hour (the following week) he was given green, blue, red, and the third hour blue, red, green. After three hours, then, each color had appeared once in each position in the hour. This made up a unit, within which the various factors of the work curve (fatigue, practice, and so on) were evenly

distributed among the colors. The next group of three would begin red, blue, green: in this way every possible order was tried. Between colors the "normal" light already mentioned illuminated the subject's room.

The number of subjects varied from one semester to another; usually there were six or eight, and two came twice a week, the rest once. Subjects who came twice a week were given one hour with the hue, and the other hour with the brightness series; the other subjects were divided up equally between the two series. Regular hours were assigned; the experimentation was done in the morning, in hour periods, between nine and one. The subjects were all trained observers, and familiar with laboratory methods and technique.

In the entire study (including the preliminary work) twenty-six persons at one time or another acted as subjects. One was a professor of psychology; seventeen were graduate students with experience in research. Eight were Harvard undergraduates doing advanced work in psychology; three of these were carrying on independent research. Three of the graduate students were women. It should, perhaps, be added that the study was made during the university years 1914-1915, 1915-1916, and 1916-1917 to the first of March.

Much difficulty was found in securing satisfactory tests, because of the fact that the purpose of the experiment prevented the employment of any test requiring use of the eyes; the purpose was not to study visual acuity with lights of different colors, or the effects of eye strain in dim light on mental work, but to discover whether *apart from such factors* different hues and different brightness had any influence on general neural tone and mental efficiency. The vast majority of tests previously used in such comparative studies (studies of the effects of drugs, for instance) were thus barred from the writer's experimentation. And much time was spent in simply developing tests which would meet these special requirements. The tests finally adopted will be described in detail later, along with the presentation of the results obtained. A large number of other tests (as dynamometer measurements, maximum speed in tapping, several forms of memory test) were tried, but found unsatisfactory and rejected.

The test routine varied, of couse, with the tests employed. The first of the work was done with the following tests, and in the following order. First, under the normal light, the subject tapped for 30 sec. (this was not the usual tapping test, but a tapping "at most comfortable rate," as will be described later). Then the normal light was thrown off and the test color on; the tapping, however, continued for another 45 sec. Introspection was now called for. After this, the multiplication was given, then the pressure test, then the memorizing. Forty-five sec. more of tapping followed this. Finally, introspection

was asked as to any change in the influence of the color from that reported when the light first appeared. The normal light was now thrown in, and the subject rested for 4 min. while the experimenter changed the colored plates, adjusted the theatre dimmer, slipped new smoked papers on the kymograph drums and adjusted the recording apparatus. Then the same programme was repeated with the other two hues. Introspection was called for only during the first two or three weeks at the beginning and end of a half year's work; for the greater part of the time the work was wholly objective.

This general arrangement was adhered to throughout the experimentation. As sufficient data were accumulated with a test it was dropped, and a new test substituted; the routine, however, remained essentially the same. Work in the color series and in the brightness series was also throughout the same; there were the same conditions, the same tests, and the same routine; so that the two series were in all respects strictly comparable.

Every half year saw a change in programme of some sort. With a new test three hours of practice were allowed, as a rule, before results were used. Data from twelve hours following this were the aim. This was not always possible, however. By the methods used, the results with a given color, in a given hour, and on a given test, obtained their significance from comparison with the results, on that test, under *all* the other lights. If now, reaction times under green were lengthened by the sound of the bell at the end of the hour then all the results, with the other colors as well, were warped in their significance. So the rule was made that no results from any hour which was interrupted, in any way, on any test, should be included. A large amount of material was, of course, eliminated by this rule; but the eliminations seemed necessary, if the subtle effects sought were not to be buried completely under such chance variations. The endeavor was also made to obtain, in each series, results from at least six individuals, on each test. This was, again, not always possible (or worth while) but was done in the majority of cases.

A word remains to be said regarding the handling and interpretation of the data. In the investigation of any such subtle influences as were being sought chance variations in the results are certain to be large, and differences (if any) due to the factors studied, small. Satisfactory interpretation and evaluation of the data are thus extremely difficult. The situation is, essentially, too complex to make possible any summary of the consistency of the findings, which could be treated statistically. Instead, a judgment upon the reliability of the data has been attempted, based upon three related, but distinct, sets of facts: (1) the consistency of the results obtained from each subject, on each single test; (2) the degree of agreement among different subjects on each test; and (3) the consistency of the results from test to test with each subject (a correlative consideration here being, of course, the consistency of the group from test to test). Any final numerical statement of the reliability of the findings must, of necessity, be partial and inadequate. Instead, an attempt has been

made to bring all the facts, considered from these three points of view, together in a judgment as to the probable meaning of the data.

The writer is not at all sure, now (1919), that a more determinate and decisive statistical treatment of the material would not have been possible. But, in defence of the procedure adopted, it may be pointed out that practically all the well-known studies having similar statistical problems have been quite as indecisive in their statistical statement. Most similar, in the statistical problem presented, are studies of the effects of drugs, of which the monographs of Hollingworth[17] and Dodge[9] may be cited. And it might be pointed out in this connection that, so far as difficulties of control and analysis are concerned, study of the effect of only three colors, in one short hour, is not a little analogous to an attempt to study the effect of three drugs in that brief space of time. The problem was not an easy one, and abounded in unusual difficulties of analysis and statistical treatment.

The methods used will be discussed in more detail in considering the results obtained with each test.

C. *Results*

1. *The Effect of Color upon Rate of Rhythmic Movement of the Finger.* This test was planned to give an indication of any change in neuro-muscular tone and tendency to movement. It was, essentially, an ergograph with minimal resistance. Clamped to the end of the table was a rod which reached obliquely in over the table top. The end was about 15 in. above the table, and the same distance in from the table end. To the rod was fastened a pulley. Through this ran a silk cord, to which was sewed a glove finger. The subject put his left fore-finger in the glove, and rested his arm easily on a support which kept his finger free above the surface of the table. The test consisted simply in moving the finger up and down with a tapping movement. The white silk cord ran over to the experimenters' table, where it was fastened to a marker which wrote on a smoked drum, and to the resistance, a single piece of ordinary rubber band. The subject was told to tap at an easy, comfortable rate, which could be kept up without fatigue during the minute tapping period; to find the rate which was most natural and comfortable for him; then to forget all about the test, letting the movement go on automatically. There was a specific caution against any effort at speed.

The test should, the writer felt, be quite highly sensitive to any energizing or stimulating effect the colors might have. If red *did* excite, or a dim light subdue, this ought to show easily in such a half-conscious rhythmic activity as this simple movement, up and down, of the finger. But there were other advantages. Practice effects were unimportant; there was no learning; there should be nothing of those

wide variations likely to appear when there is any requirement of maximal effort.

The place of the test in the routine has already been described. The tapping was begun 30 sec. before the stimulus light was turned on, and was continued for 45 sec. after this. The other tests were then given. And finally, just before the stimulus light was turned off, there was tapping again for another 45 sec. Time was marked off in 15 sec. units. The results were handled in terms of these units. The number of taps in the first two quarter-minutes under the normal light were averaged to give the average normal for the period. The number of taps in each one of the six quarter-minutes under the stimulus light were then scored so much *plus* or *minus* from this normal for the period. And the difference between the variation from its normal produced by red, and the variation from *its* normal produced by the blue, gave the difference in effect of these two colors.

This final figure, it should be emphasized, was almost wholly free from the influence of constant errors. With some of the tests, systematic variation through the hour, due chiefly to difficulty in getting started with the work, and to fatigue in the last period, were considerable. However, with this arrangement of the tapping, the normal, for each period, just preceded the color period itself. If red came last in the hour on a given day, its normal came near the last too; the normal took account of the variations through the hour (as well as of the daily variations), and none of these irrelevant factors got itself into the results.

In handling the results the number of taps, in each 15 sec. under the color, was first scored as so much *plus* or *minus* from the average number of taps per 15 sec. under the normal light, as has been just described. The variations for the first three quarter-minutes under the color were then added, giving the total variation for the first 45 sec.; and the variations for the last three quarter-minutes also added. For instance, suppose the number of taps per 15 sec. ran as follows:

	Green									
normal		first 45 sec.						last 45 sec.		
17	15	16	17	19	(other tests)			17	19	20

This would be scored

16		0	1	3	(4)			1	3	4 (8)

To obtain the final percentage statement, the average normal was multiplied by three, to give the average normal for 45 sec. and the succeeding variations per 45 sec. stated as per cents of this. The above record would thus become

		Green	
	normal	first 45 sec.	second 45 sec.
	48	4	8
and in per cents	48	8%	17%

This could, of course, have been done less elaborately by running a normal of 45 sec. instead of 30 sec. But since the work was begun with a shorter normal it was so continued, and the above method of handling used.

TABLE I

TAPPING RATES: IN TERMS OF % VARIATION FROM "NORMAL"

Four Brightness Series

Sub.	Av. Nor. 45″	I 1st 45″, av.	I 1st 45″, m.v.	I Last av.	I Last m.v.	II 1st 45″, av.	II 1st 45″, m.v.	II Last av.	II Last m.v.	III 1st 45″, av.	III 1st 45″, m.v.	III Last av.	III Last m.v.	IV 1st 45″, av.	IV 1st 45″, m.v.	IV Last av.	IV Last m.v.
And.....	46	5	6	5	11	— 2	4	0	10	— 3	6	—1	11	2	10	— 6	10
Dav.....	55	—2	3	13	5	1	3	3	7	0	2	2	11	2	4	1	9
Fin.....	49	—8	6	—6	9	—12	11	—1	10	— 5	3	—2	4	—12	6	—11	10
Kal.....	102	7	6	12	14	5	7	10	13	— 2	7	3	20	—11	9	— 5	20
Average	...	.5	5	6	10	— 2	6	3	10	—2.5	4	.5	11	— 5	7	— 5	12

(Ten hours each subject.)

Three Brightness Series

Sub.	Av. Nor.	I 1st 45′ av.	I 1st 45′ m.v.	I Last av.	I Last m.v.	II 1st 45′ av.	II 1st 45′ mv.	II Last av.	II Last m.v.	III 1st 45′, av.	III 1st 45′, m.v.	III Last av.	III Last m.v.
And.....	62	16	7	18	'12	2	5	13	9	0	8	5	9
Cur.....	68	4	5	9	6	6	3	2	8	2	3	11	12
Dav.....	77	7	8	9	4	0	2	6	10	7	10	4	7
Woolb.....	51	1	5	0	7	3	3	—4	8	0	3	— 7	10
Mor.....	124	6	7	12	8	11	10	16	11	7	5	18	13
Average.....	...	7	6	10	7	4	5	7	9	3	6	6	10

(For each subject in order the hours were 20, 10, 8, 9, 9.)

TABTE I—*Continued*

Five Hue Series

Subj.	Av. N.	Red av.	Red m.v.	Red av.	Red m.v.	Yellow av.	Yellow m.v.	Yellow av.	Yellow m.v.	Green av.	Green m.v.	Green av.	Green m.v.	Blue av.	Blue m.v.	Blue av.	Blue m.v.	White av.	White m.v.	White av.	White m.v.
Br	93	2	4	8	3	2	3	4	2	—2	3	0	6	2	2	0	3	—2	2	5	5
Ch	84	—3	6	6	9	0	6	5	2	—2	3	2	5	1	6	2	7	0	2	10	4
Da	57	0	5	9	8	0	5	1	7	2	2	12	6	0	4	1	8	—1	3	0	7
Ohl	73	3	4	6	4	3	7	0	6	2	4	3	4	0	5	3	4	3	5	1	7
La	109	—2	4	7	7	2	3	10	5	0	2	10	6	1	4	4	4	—1	3	4	6
Average	...	0	5	7	6	1	5	4	4	0	3	5	5	1	4	2	5	0	3	4	6

(Ten hours each subject.)

Three Hue Series

Subj.	Av. N.	Red av.	Red m.v.	Red av.	Red m.v.	Green av.	Green m.v.	Green av.	Green m.v.	Blue av.	Blue m.v.	Blue av.	Blue m.v.
Allp	147	6	4	4	7	6	3	6	7	3	6	2	6
La	107	2	4	6	6	1	3	4	6	1	2	9	9
Bul	44	3	4	9	8	2	5	1	12	3	4	6	8
Br	148	3	2	5	3	—2	2	—1	3	—1	3	3	3
Ch	106	3	4	2	14	0	3	12	8	0	3	4	12
Wo	49	—2	2	—6	11	1	3	1	9	0	4	1	7
Sp	60	10	.	1	14	1.5	12	6	10	0	4	9	6
Average	...	4	3	3	9	1	4	4	8	1	4	5	7

(Twelve hours three subjects, nine hours four subjects.)

The m. v. after the group average is the average of the m. v.'s, not the m. v. of the averages. In handling the per cents, fractions of one per cent were read as the nearest whole number.

The results for the brightness series are given in the following table (Table I). They are stated, as has just been described, as per cent variation, from average under normal light for 45 sec., for 45 sec. under the color. The first series was run with four lights, bright, moderately bright, medium and dim. In this series were four subjects; the number of hours for each was ten. In the second series three brightnesses were used; the first and the last were the same as in the first series, but in place of the moderately bright and medium light a new medium, the 32 c. p. Mazda, was substituted. In this series were five subjects, two of whom had also been in the previous series. The number of hours with one subject was twenty, with one ten, with one eight, and with two nine.

The results may be summarized as follows. Of seven subjects five, including those from whom there are most results, show an increase in rate of tapping with bright light, and a decrease in dim light. One subject showed a slight, the other a more considerable, reversal of these results. In the case of this last subject, there is some evidence to suggest a special, somewhat morbid, peculiarity in this respect. For those subjects giving positive findings there was not only an increase under bright light, but a decrease under the dim light as compared with rate under the medium light. There was also no evidence of decrease of these positive findings through a semester; one subject, kept at the test for a year and a half with a view to obtaining evidence on this question, gave as positive results at the last as at the beginning.

It must be emphasized that the results from the two tapping periods, the first 45 sec. and the last 45 sec., have a very different significance as regards the possible effect of the lights. Any change in the first period, when the light had just been thrown on, might be due simply to a shift of attention, or some similar transitory element in the situation. The results from the last 45 sec. should be of much more importance, since the stimulus light has been on, by that time, some 12 min., and any effect then observed must be of some permanence.

As a matter of fact, the results are most distinctive in this last period. The group averages show greatest number of taps for the brightest light, and a regular and even decrease down to the least number for the dimmest light. When the groups are analyzed it appears, in the first place, that the two subjects who worked in both series agree substantially, in the indications they give under the two somewhat different conditions. In the first group of four (with four lights) all the subjects agree in showing most rapid tapping with the bright light, slowest with the dim. In the second group of five, three subjects give this same result. One, however, shows a slight, the other a fairly distinct increase with the dim light over the bright one. In this last case, introspection may throw some light on the situation. This subject (Mor.) was extremely fond of dim lighting. This was

due, he said, to a general liking for subdued light effects. He was fond of sitting in his room in the dark, or with the lights low, was very fond of candle-light, could think best when the lighting was dim. With the other subject giving opposed results there is no such clear-cut preference, though he did say once or twice that he could think better in the dark.

The results with the hues may be very briefly summarized. No differential effect of different hues, at equal brightnesses, upon rate of tapping appears. Two series were run, one consisting of red, yellow, green, blue, white, the other of red, green, blue. Eight subjects took part, five in the first series, four of these and three more in the second. It may be said, shortly, that no consistent findings appear with regard to any one of the hues studied. (See Table I.)

2. *The Effect of Color upon* (*a*) *Pulse and Respiration,* (*b*) *Estimates of Pressure, and* (*c*) *Judgments of the Pleasantness of Touch Substances.* It may be said at once that the work with pulse and respiration gave results which were entirely negative. The work was very carefully done; records were taken both at the beginning and the end of the color period, and studied with reference to both rate and shape of curve. But nothing appeared which could be correlated, in any way, with either brightness or hue.

The experiment of Stefanescu-Goanga,[85] already mentioned, was also largely repeated, with the writer's setting, but with negative results.

These negative findings are not, of course, proof positive of lack of any effect of hue or brightness upon the physiological processes. There may be an effect, and of a cumulative importance, on respiration and pulse. But the changes under such conditions must be, within the time any convenient measurement might extend (30, 45, or even 60 sec.) so minute as to be probably below the threshold of even the best technique.

It may also be said, with equal brevity, that results with the "pressure" and "pleasantness" tests were similarly negative. No influence of either brightness or hue, upon performance with these two tests, could be discovered.

The apparatus for the "pressure" test was simply an ordinary postage scale. For 1 min., during the 5 min. intermission between colors, the subject practiced pressing down exactly 16 oz., while looking at the dial. The scale was then taken away, but presented again, when the time during the color period came for the test; the subject was now given five trials at pressing down 16 oz. as previously practiced, but this time with the dial turned away from him. The record taken was the amount actually pressed at each attempt (the amounts pressed in the five trials being summed to make the "pressure" score for each color).

Just what function the test might be supposed to sample was not clear. Perhaps a bright light, or a red, if stimulating, would bring

about a more energetic push on the scale, and an overestimate. And a dim light might bring about less activity. But the fact was that no such relationships appeared. The test was interesting, but of no value, at least for the writer's experiment.

The experimentation with the touch substances was almost as simple and direct. A large variety of touch substances (different kinds of cloth, wood, metal, and so on) were fastened to small blocks of wood (one and one-fourth by two in. and one-half in. thick). Twenty of these blocks were put in a shallow box just big enough to hold them, five in a row with four rows. This box was covered with a hood of the heavy, unglazed drawing paper. When the time came for this test, the box was put on the table in front of the subject who, with his right hand, beginning at the upper left-hand corner, worked across the top row to the right. Each substance was felt in turn, and its pleasantness expressed on a scale of seven, where one was most unpleasant, four indifferent, and seven most pleasant. After the top row was finished, the subject dropped his hand to the next row, working from right to left; and so on till the twenty substances were all judged. The hood hid the substances from view, so that the subject had only a touch acquaintance with the materials.

After the test was finished the box was taken from the table, and the other tests given. During the intermission between color periods the experimenter, by a simple arrangement, completely and systematically changed the order of the substances. And from week to week the substances used were changed. The purpose in each case was to break up any memory of previous judgments on a particular substance, to make each estimate a product simply of the two present factors, the touch substance as it felt now, and the present affective attitude caused by the color. The results were handled by simply adding the values assigned to the twenty substances under each color, giving a total which might, perhaps, be called the affective level under the given light. The test seemed to the writer to have possibilities. But the findings were altogether negative. He still feels, though, that something of this sort might yield interesting data in a study of some other problem, to which it was better adapted.

3. *The Effect of Color upon Rate of Multiplying.* No study of this type would be complete without some sort of arithmetical test. Such tests seemed almost impossible of control sufficient for the purposes of the present problem. Little help could be obtained from previous work, since practically all tests previously used were ruled out, from the start, by the requirement that there should be no visual presentation. Nevertheless, the writer felt that an adequate study of his problem required at least an attempt at measurement of possible effects of color upon some such relatively complex form of mental work.

The test, as finally made up, consisted of multiplications of one-place by two-place numbers. Eleven such multiplications were given with each color; a test sheet, for a given day, thus consisted of three rows each having eleven problems. Three such sheets were prepared, enough for three weeks (or

a single series, where the colors came each once first in the hour). The three were then repeated in order. But the order of the colors was varied, so that any difference in difficulty of the different groups of eleven was distributed. The record was in time for each multiplication, as taken with a stop-watch, and in errors. In handling the material, that multiplication of the eleven which had the longest time was struck out; quite frequently a time would run much beyond the average as a result of some slight distraction; an elimination of the longest time of each eleven allowed, to some extent, for these variations. The times for the remaining ten were summed.

TABLE II

TIME IN MULTIPLYING (BRIGHTNESS SERIES). IN TERMS OF % OF AVERAGE TIME FOR HOURS

Four Brightness Series

	I. (bright)		II.		III.		IV. (dim)	
Subject	av.	m.v.	av.	m.v.	av.	m.v.	av.	m.v.
And	89	13.0	101	13.0	106	13.0	103	8.0
Dav	96	5.6	106	9.5	101	7.8	97	9.8
Fin	97	3.5	99	7.5	104	9.0	99	8.0
Kal	99	9.0	94	11.0	97	5.0	109	6.0
Average	95	7.7	100	10.2	102	8.7	102	7.9

(Ten hours each subject.)

Three Brightness Series

	I.		II.		III.	
And	100	6.2	99	4.7	101	5.6
Cur	95	7.6	101	10.0	104	7.6
Dav	99	4.9	99	4.8	102	5.3
Woolb	97	7.0	102	3.9	100	6.3
Mor	100	9.5	100	9.3	100	40.1
Average	98	7.0	100	6.5	101	6.9

(For each subject in order the number of hours is 10, 10, 8, 9, 9.)

TIME IN MULTIPLYING (HUE SERIES). IN TERMS OF % OF AVERAGE TIME FOR HOUR

	Red		Green		Blue	
Subject	av.	m.v.	av.	m.v.	av.	m.v.
Allp	99	4.7	101	6.2	99	8.2
La	101	4.4	102	4.5	97	2.7
Bul	100	8.3	99	7.9	100	8.8
Br	103	6.4	98	5.6	99	3.6
Cha	100	6.5	98	5.8	101	10.2
Woolb	98	7.5	104	7.8	97	12.0
Tul	103	4.4	102	6.1	95	6.0
Average	101	6.0	101	6.2	98	7.3

(Twelve hours three subjects, nine hours four subjects. m.v. after the group average is, for each table, the average of the m.v.'s, not the m.v. of the averages.)

The sets of eleven were made up to be as equivalent as possible. Any three numbers, as 6, 7, 9, can obviously be combined to form six problems of the type used: 6 times 79, 6 times 97, 7 times 69, 7 times 96, and so on. Five and a half such groups of six would, therefore, make up the materials for a day's test sheet: eleven problems with each of three colors. The problems were distributed among the three colors to give as great variety as possible, and as great uniformity: if 7 was used twice with a color as multiplier those two multiplications did not come together, and there were two sevens as multipliers with each of the other colors. In giving the test, the experimenter simply read each problem to the subject, slowly and distinctly, as "six times eighty-seven." The subject did the work mentally, and then replied with the answer, "five twenty-two." And the experimenter took time from the last syllable he said in giving the problem to the last syllable of the subject's reply.

In combining the results for a half year or more, two important factors had to be taken into account: (1) variations within the hour, and (2) variations from one experimental hour to another through the year. In dealing with practice through the year, some percentage statement of ranking of the colors, within the hour, was naturally suggested. The method finally adopted employed, as norm for the hour, the average of work under all three colors. The time under each color was then rated as a per cent of this.

For instance, suppose on a given day subject A's time for the ten multiplications, under each one of the three colors, ran as follows:

red	green	blue
55.0	57.2	59.2

these three would be averaged, giving 57.1., and per cents taken from this making a score of

red	green	blue
96%	100%	104%

Averages of these per cents through the year gave results free from practice effects from hour to hour. They were free from the many other variations from week to week, due to changes in condition, previous fatigue, and so on. Finally, the results from individual to individual were directly comparable; the average actual time for some individuals was over twice that for others.

The results are summarized in Table II. Mor., who gave results contrary to the tendency of the group in the tapping, does the same here. Otherwise there is a fairly consistent slowing with dim light, and acceleration of work with the bright.

The hue series gave no results so consistent. There is a suggestion of most rapid work with blue and slowest with green. But inspection of the individual averages reveals no consistent tendency such as appears with the brightnesses; minimum rate occurs three times in green, twice in red, once under blue, red and blue are once "tied for first place."

4. *The Effect of Color upon (a) Rate of Free Association, and (b) Immediate Memory for Nonsense Syllables.* The arithmetical work called for associational processes of a rigidly controlled and limited sort. As a contrast to this, it seemed interesting to try what the effect of color might be upon wholly uncontrolled, free association. Some test of memory also seemed necessary for a rounded test programme. Memory for nonsense syllables and rate in free association, under the various hues and brightnesses, were therefore investigated. And, it may be said at once, the results were entirely negative.

The materials and method for the free association test were substantially the same as used by Professor Langfeld in his study of the effects of prolonged fasting.[19] His lists of words, twenty to a list, were used, twenty words with each color. Timing was with a stopwatch. The experimenter simply read each word, slowly and distinctly, to the subject, who was instructed to reply at once with the first word that came into his mind. Time was from the last syllable of the stimulus word to the last syllable of the response. In handling the results, the longest and the shortest times were struck out, as most likely to contain extreme chance variations, the longest as perhaps due to a complex, the shortest as due, perhaps, to a mere perseveration from some previous word or associate. The total for the remaining eighteen words was then found.

The memory test was quite as simple. Immediate memory was clearly the only type which could be controlled adequately for the purposes of the experiment. Various kinds of material were tried; nonsense syllables were found to be the best suited to the test programme. Ten syllables were used with each color; a sample list runs as follows:

zok dib niv bam zek miz niz dop lor tem

In making the lists no long vowels were used (since these were found to "cling" in the memory more), and various other more usual precautions were taken, to make the lists of even and equivalent difficulty.

In giving the syllables they were read at a rate of two per sec., with a grouping in pairs by inflection of the voice. After the first reading the subject was asked to "give back" as many as he could. The list was then read to him again, and he was again asked to repeat all he could remember. This was done twice more, making four readings in all with each list. The syllables were scored as correct or half correct, in the recall; the final record was the sum of the recalls after each of the four readings. For instance, under red, the number of syllables given back after the first reading might be four, after the second five and a half, after the third seven, after the last eight. The record under the red would then be 24.5. The test was a great bore to the subjects, and yielded results of no value.

5. *The Effect of Color upon Rate of Continuous Choice Reaction.* A measurement frequently obtained, in work of this general nature, is speed in choice reaction. The writer felt strongly, however, that the classical reaction-time experi-

ment was neither suited for inclusion in his test programme nor calculated to get at the effects which were being sought.

It was at first planned to use the standard method, two stimuli and two keys, and the time in sigma for each separate reaction. But the technique was considerable; much time would be required to accumulate a small amount of data; and the measurements would be so minute that the appearance in them of so subtle an effect as the colors might be expected to have would hardly be likely. In fact, such investigation, with negative results, had already been reported.[23]

Besides, the writer had a feeling that the effect of a bright light, for instance, might consist not so much in an actual stimulation as in a "sustaining" of any activity which might be going on. Such an influence should show best on some form of rather monotonous work which could go on for an appreciable length of time in routine fashion; something comparable to work in a factory, where there are a few well understood stimuli and half-automatic responses, occurring over and over. Rivers[31] and Hollingworth[17] both used typewriting. Typewriting could obviously not be used in the present study; but something of this general nature, a continuous series of reactions to simple stimuli, was the type of task desired.

It finally occurred to the writer that instead of single reactions, and a time measurement of each, a continuous series, with timing only of the whole series, could be arranged; such a test would give exactly the sustained routine activity desired, and would also accommodate itself much more easily to the total test programme. The test, as finally worked out, may be briefly described. For signals two telegraph sounders were used; one, giving a sharp, loud click, to the right of the subject's table; the other, sounding duller and less loud, to his left. On the table, mounted on a small block of wood about 5 by 9 in. in size, were two keys, close enough together to be operated by the first and second fingers of the subject's right hand. The subject was simply to press the right-hand key when the right-hand signal sounded, and to respond with the other key to the left-hand signal. And the apparatus was so arranged that correct response on the part of the subject to a given signal automatically brought about the presentation of the next signal. But if the subject responded incorrectly, pressed the wrong key, no new signal appeared, and the subject had to rectify his mistake before he could proceed.

The test thus consisted of a continuous series of choice reactions, the subject setting his own rate and the apparatus exactly keeping pace with him, presenting new signals as fast as the old were reacted to. The test was run 4 min. with each color, 2 min. at the beginning of a color period and 2 min. at the end (taking the place of the tapping test, after work with the tapping test was completed, in the routine).

Highest possible speed on the part of the subject was not desired; he was told rather to work along steadily, much as he would in running a typewriter, and to emphasize accuracy rather than speed. Time was marked in 30 sec. intervals; both time and correct reactions were recorded by electric markers on a smoked drum. Only two choices were used, in order that learning might be easy and practice effects not great, and mistakes not over numerous.

The important, and original, feature of the test was, of course, the apparatus by which reactions and stimuli were thus interrelated. This interrelation was brought about by means of a special adjustment between contacts with each signal and with each key, and contacts on a revolving wheel. The wheel consisted of two brass cog wheels soldered together; bearing on the right hand wheel were the contact connecting with the right-hand signal and the contact connecting with the key to be pressed when the right-hand signal sounded, and bearing on the left-hand wheel were the contact to the left-hand signal and the contact to the key to be pressed when the left-hand signal sounded. The key contacts were side by side (that is, bearing on cogs which were side by side on the wheel); and underneath, making on the next cogs below, were the two signal contacts, also side by side, that is, making on cogs which were side by side. Certain cogs on each wheel were filed out, however, and these were so chosen that one, and only

TABLE III

NUMBER OF CORRECT CHOICE REACTIONS IN TWO MINUTES. IN TERMS OF % OF AVERAGE NUMBER, PER TWO MINUTES, FOR THE HOUR

Brightness Series

Sub.	I. (bright) First 2′ av.	I. (bright) First 2′ m.v.	I. (bright) Second 2′ av.	I. (bright) Second 2′ m.v.	II. First 2′ av.	II. First 2′ m.v.	II. Second 2′ av.	II. Second 2′ m.v.	III. (dim) First 2′ av.	III. (dim) First 2′ m.v.	III. (dim) Second 2′ av.	III. (dim) Second 2′ m.v.
And.....	100	2.0	102	6.9	100	8.5	104	8.9	95	7.4	98	7.6
Cur.....	98	5.5	102	5.0	107	5.2	105	2.8	91	8.3	98	7.1
Dav.....	103	4.1	99	3.3	100	4.8	99	4.5	101	4.6	96	6.6
Pep.....	101	2.0	101	4.0	101	1.0	100	4.0	99	2.0	98	4.0
Mor.....	99	4.2	99	5.9	98	5.2	102	3.9	99	6.2	102	3.9
Woo.....	97	3.4	101	2.5	100	5.9	100	4.0	98	2.9	103	4.3
Av.....	99	3.5	101	4.6	101	5.1	102	4.7	97	5.2	99	5.6

(Twelve hours each subject.)

Hue Series

Sub.	Red First 2′ av.	Red First 2′ m.v.	Red Second 2′ av.	Red Second 2′ m.v.	Green First 2′ av.	Green First 2′ m.v.	Green Second 2′ av.	Green Second 2′ m.v.	Blue First 2′ av.	Blue First 2′ m.v.	Blue Second 2′ av.	Blue Second 2′ m.v.
Allp.....	100	2.9	102	1.7	100	4.6	103	2.0	96	4.7	99	2.1
Br.....	101	2.5	98	1.6	99	2.5	100	2.1	101	3.1	99	3.1
Bul.....	100	4.5	101	2.5	101	3.1	103	3.3	104	5.3	91	6.1
Chac.....	102	3.1	99	2.1	98	3.2	100	3.3	99	2.9	101	1.6
Tul.....	102	3.0	100	3.0	102	2.0	99	3.0	102	2.0	96	3.0
Woo.....	95	4.2	103	2.0	95	6.0	106	3.9	100	3.9	101	4.4
Av.....	100	3.4	100	2.1	99	3.6	102	2.9	100	3.6	98	3.4

(Twelve hours each subject.)

The m. v. in the average row is the average m. v., not the m.v. of the averages.

In handling per cents values up to .5% were dropped.

one, cog was presented at any given point. The result was that one, and only one, signal contact, and one and only one key contact, were making with the entire wheel at any given time.

Suppose, then, the subject responds (correctly) to a previous signal by pressing the proper key, thus closing the circuit, and suppose the right-hand signal contact is making with a cog on the wheel. The right-hand signal will be sounded. At the same time the current will go through a magnet, jerking forward a rachet to engage a succeeding cog on the wheel, so that when the subject breaks the circuit, by releasing the key, a spring action against the magnet will jerk the wheel around one cog's distance. The result will be that the cog which was making with the right-hand signal contact will now be making with the right-hand key contact. And only by pressing the right-hand key can the subject again close the circuit, and proceed further with the reaction series.

The essential feature of the device thus consists of this arrangement by which a given contact on the wheel makes successively with a signal and a key contact, so bringing it about that the reaction called for is the only one which can be completed.* The apparatus, which was mounted on a base about 4 by 12 in. (it could have been built half this size) and stood about 3 in. high, was in a sound-proof box under the experimenter's table. The cogs made in irregular order, so that the subject was given signals sometimes alternately right and left, again two or three in succession left or right, and in various combinations. After some five months of practice one subject began to learn the combinations; the connections were then simply reversed, and so all the combinations changed. Aside from this, there were no evidences of marked practice effects.

The results were first handled simply as total number of correct reactions in the first and the last 2 min.; this was taken as a measure of what might be called "reaction efficiency" under the given conditions. However, since individual differences, and variations from day to day, were marked these figures were converted into per cents of the average for the day, as were the times in the multiplication test.

It should be mentioned that 1 min. practice was given before the hour began; there was nevertheless evidence of practice effects through the hour, especially with certain subjects. And since no convenient way was found for eliminating such constant errors from the results

* The apparatus can be made to handle three, four or more choices, simply by using three, four or more cog wheels with their pairs of contacts. It can be used with any type of stimulus which can be electrically operated, and with any type of reaction. The arrangement would, therefore, seem of some general usefulness. The mechanism used by the writer was experimented with up to speeds about twice those of the average subject, and found perfectly accurate at such rates.

As has been pointed out to the writer by Prof. W. F. Dearborn, the test is somewhat similar to Seashore's "Psychergograph;"[21] the apparatus above described is, however, much more practicable. A mechanism very similar to the writer's has recently been described by McComas.[22]

they may be supposed to make the mean variation somewhat larger than it should be.

In the brightness series the group average shows "reaction efficiency" to be greatest under the medium light, least under the dim. As before, Mor. is an exception, and this time also Wool. Mean variations are also slightly greater under the dim light. Differences between first and last 2 min. periods are hardly consistent or definite enough to allow any significance to be attached to them; the two periods are probably best considered together. In the hue series the group average is least with blue; red and green show no important difference. It can hardly be said that in either series the results are of any significance, except in so far as certain subjects in the brightness series give findings in agreement with their previous records on the tapping and arithmetic tests. But this consistency would seem of some possible importance.

6. *The Effects of Color, as given in the Introspective Reports.* As has already been said, for the major part of the experimentation no introspection was called for. Introspection was obtained, however, for the first and last hours of each term, in order to study (1) group tendencies and the amount of individual variations in report, and (2) the effects of habituation to the colors. The tables are too complicated to present; the findings may, however, be briefly summarized.

In this work introspection in terms of the Wundtian tridimensional theory was called for.

This was done only after considerable hesitation. But more of a description than a mere indication as to pleasure value was wanted. And if each subject was left to form his own categories, tabular statement and cross-comparison would have been difficult. The subjects were therefore asked, for a time, to report on the colors as pleasant or unpleasant, exciting or depressing, tensing or relaxing. Two degrees of each quality were recognized; a color could be pleasant or it could be very pleasant, or it could be indifferent, neither pleasant nor unpleasant. The work was done with the four-color brightness and the five-color hue series, and was continued four weeks with the first and five with the second (making up on each case one "unit" where each color occurs once in each position through the hour).

The group totals show, for the brightness series, the two bright lights as most pleasant, the dim least. The bright lights are most exacting, tensing; the dim is most depressing, relaxing; indifference centers with the medium light. All of this was to be expected, except perhaps the pleasantness of the very bright light, which was not found unpleasant once in spite of its brightness. In the hue series, green was most pleasant, white next, blue most unpleasant. Red is most exciting and tensing, blue and white are most often called

depressing and relaxing. Judgments of indifference are rarest under red, showing that this hue is usually taken positively, one way or the other.

The variability of a given individual, from one hour to another, was considerable. For instance, And. reports the medium-bright light twice as very pleasant, and twice as wholly indifferent; Cha. finds blue three times exciting, once very exciting, once depressing; Bul. finds white tensing once, relaxing twice, indifferent twice; Lang. finds blue pleasant twice, unpleasant once, indifferent twice.

Individual averages also differ markedly. Bul. finds all the hues and all the brightnesses uniformly pleasant, with green and the brightest light each called very pleasant once. The dim light is for Fin. and Dav. uniformly unpleasant. Cha. finds red very pleasant, Ohl. very unpleasant. Red is for Bul. relaxing, for Dav. very tensing. The results of continued experience with the colors were quite what would be expected. There was a thinning of the feeling, a dropping-away of associative enrichment and toning, a matter-of-course attitude which paid very little attention to the colors one way or another.

Thus one individual, in the first weeks of his experience as subject, reported the bright light as "exhilarating, hard on the eyes at first, but less hard than the dim light," and later in the same period, "pleasant, more awake." The dim light was disagreeable, unpleasant, sleepy, tensing, bothering to the eyes. After three terms with the experiment he speaks of the bright light as "just an ordinary light, a bit pleasant when first turned on, doesn't mind it at all." And at the end of the period he says that there "is nothing special to report, slightly pleasant, that's all." Another subject reports in the first weeks that the bright light is "exciting, livening, like sunlight," the dim is "easy on the eyes, relaxing; doesn't feel like working." At the end of the year introspection on the bright light is simply that "the dirt spots on the paper show up more;" the dim light "tends to put to sleep, a little depressing."

Similarly, in the hue series, at the beginning of the year, a subject reports red "very pleasant, because of its richness," and again, "very pleasant, it is so rich." Blue is "pleasant, reminds one of the sky and clouds," again is "ghastly, unnatural, unpleasant, exciting" (this the following week!). At the end of the year blue is "neutral" and again "neutral in all ways." Red is "perhaps slightly pleasant" and again "slightly pleasant: it came as something of a surprise." Another subject who at first "didn't like the red a bit" and again found it "decidedly unpleasant, tensing, exciting," toward the end of the year called it merely "not quite so pleasant as the others," and "on the pleasant side." Blue was at first "very unpleasant, disagreeable, tiresome;" by June it had become "quite pleasant, rather restful," and again "quite pleasant, rather restful."

It may be said shortly that there is, if the introspective reports may be trusted, a definite decrease in affective reaction to both hue and brightness, with habituation.

The results obtained in the systematic experimentation may, then, be briefly summarized.

(1) Results with a tapping test, a multiplication test, and a continuous reaction test, suggest a decrease in function under dim, and an increase under bright light. With the other tests no effect of brightness was observable.

(2) The objective measurements showed no effect of hue, independent of brightness, upon the functions tested.

(3) Introspective estimates indicated marked variability, and marked individual differences, in the feeling-attitude toward brightness, and especially toward hue. A marked decrease in affective reaction to both hue and brightness, with habituation, is the only feature common to all the introspective reports.

IV. Summary and Conclusions

So much, then, for the writer's experimentation. It remains to evaluate these findings in the light of previous work, and to summarize what seem to be the most probable conclusions with regard to the problem.

First to be considered (1) are possible differences among the hues, in their effects upon mental and physical activity. As has already been intimated, the writer feels that there is no adequate evidence in previous work to show any such differences. He has found no such differences in the present experimentation. The common notion that certain hues have a marked influence upon emotional tone and mental work is the result, he believes, of such subtle and pervasive influences, arising from figures of speech, custom, artistic and social convention, and everyday association of certain hues to other sensations of a strong affective toning, as have already been mentioned in the historical review. The misconception may also be due in part to a failure to differentiate the influence of brightness from the influence of hue.

If there is an effect of illumination upon mental work, it would appear, then, (2) that brightness must be the important factor. Certain of the experimental results obtained by the writer have suggested a slowing of mental work under dim light and a stimulation under bright light. Such an effect is at least not incompatible with what little previous work has been done in this field; and the hypothesis of a stimulat-

ing or dynamogenic influence of brightness upon mental work and neuro-muscular tone seems by no means unreasonable. It need hardly be added that, if there *is* such an influence, it is a matter of great general importance, from a practical point of view.

It must, however, be emphasized at once that the writer's positive findings are few and far between, and that their reliability is by no means all that could be desired. The writer has argued that in such a study the interrelation of all the results should be taken into consideration, in judging of the significance of the findings. And he has based his conclusions on (1) the consistency of the results obtained from each subject on each single test, (2) the appreciable agreement among different subjects on the same test, and (3) the consistency of the results from test to test with each subject. But even with such a liberal basis of interpretation, it can hardly be said that the results do more than "suggest" the inferences which have been drawn.

It is also obvious that fifteen minutes of experimentation in a laboratory dark-room tell us little about what might be the effect of the same brightness continued throughout the day, in shop or schoolroom. Fundamentally, the problem is not a laboratory problem, and needs study of a larger sort, in schoolroom or shop. The present study can be thought of as contributing little more than a definition of the problem, and a suggestion of methods. But the writer is convinced that there is here a real problem in applied psychology, well worth more extended study.*

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* But not, let it be repeated, in the laboratory! The writer finds himself, after three years of intensive research with mental tests, going over this manuscript with oddly mixed feelings. Not so long ago he returned from a four days' visit to a city of about 11,000 population: during those four days, he personally tested every child of the 1,500 children in that school system. And in contrast, three years of weary, discouraged puttering in a dark room, with a total of twenty-six subjects! He would hardly think of basing any very serious conclusion, now, on less than that many hundred. But by all this he does not mean to imply a loss of interest in the older type of experimental problem. Rather he believes that practically all of the laboratory problems can be handled, in the large, by the "test" method, with such massed cases. He is convinced that recent developments in the field of group-tests are much more important for experimental psychology than is usually realized. The group-test is really, he believes, a new method, of splendid promise, applicable to the entire range of psychological investigation. And it is to the "group-test" that he looks for a final conclusive treatment of the majority of the older experimental problems: that is, a majority of those problems which may be worth such treatment!

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THE NATURE OF THE AFFECTIVE JUDGMENT IN THE METHOD OF PAIRED COMPARISONS[1]

By M. Yokoyama

In a previous article the writer has reported data which show the manner in which judgments of pleasantness are dependent upon the simultaneous operation of the color and the form of geometrical figures cut from colored paper.[2] These results were obtained by the method of paired comparisons, the most usual tool in the experimental study of affection. In this investigation a great many introspective reports were obtained from observers at various stages of practice but were not fully presented for the reason that they threw but little light upon the immediate problem. These protocols, however, bear quite intimately upon the nature of the material which the method of paired comparisons yields, and it is proposed in the present paper to study them with especial regard to the nature of the affective judgment.

Titchener in 1908, discussing the nature of affection in the light of the experimental work available at that time, concluded that affection could be neither sensation nor attribute of sensation.[3] Since then, however, the face of introspective psychology has been changed by the realization of the need for a constant differentiation between mental process on the one hand and meaning on the other.[4] Young noted how affective judgments may be meanings of preference, not dependent directly upon affective process;[5] but he was not concerned with

[1] From the Psychological Laboratory of Clark University. The present paper is abbreviated for publication by the omission of many introspective reports. A bound manuscript containing detailed protocols will be found under the same title in the Clark University Library and may be borrowed under the usual courtesies of library exchange.

[2] M. Yokoyama, Affective tendency as conditioned by color and form, *Amer. J. Psychol.*, 1921, *32*, 81-107.

[3] E. B. Titchener, *Psychology of Feeling and Attention*, 1908, 1-168, esp. 33-78, 167 f.

[4] Orth's distinction in 1903 between feelings and *Bewusstseinslagen* was, of course, prior to the full recognition of the relation of meaning to conscious attitude: J. Orth, *Gefühl und Bewusstseinslage*, 1903.

[5] P. T. Young, An experimental study of mixed feelings, *Amer. J. Psychol.*, 1918, *29*, 237-271, esp. 252-258: *cf.* also C. E. Kellogg, Alternation and interference of feelings, *Psychol. Monog.*, 1915, No. 79, esp. p. 89.

raising the fundamental issue, and sought primarily to rule out from his results a class of cases that was of a different order from those with which he was mainly concerned. The present writer seeks to raise the major question whether the method of paired comparisons can ever be relied upon to yield material other than affective meanings. He has no hope of settling finally the issue of the existential nature of affection, but believes that a demonstration of the inapplicability of this orthodox method to an experimental study of affective processes is worth the making. Certainly, so it seems to him, it is no longer possible for the experimental psychologist to assume that the mere utilization of a method assures results that exhibit laws of affective process.[6]

Observers

Our *O*'s were peculiarly well qualified to bring out distinctions between meaning and process, because they had been sensitized in the 'atmosphere' of the Clark Laboratory to the process-nature of introspective material and to a persistent distinction between *Beschreibung* and *Kundgabe*. On the other hand, the very fact that the problem of meaning is so much 'in the air' at Clark may perhaps be thought to have predisposed them unfairly toward the discovery of meanings. However, without some such sensitization, which amounts to little more than a training in the refinement of modern introspection, it would have been hopeless to seek results which bear upon our problem at all.

An *O*, in approaching this problem, is apt to establish by his protocols his own theoretical bias. Hence we note here these biases, as indicated in formal statements by the *O*'s themselves. Observer B, the director of the laboratory, stated that for a number of years he had been sceptical of affection as an existential mental process and had been becoming constantly more convinced that it could be adequately accounted for as meaning or act. Observer D, a psychologist of some years standing, stated that she began the experiment without definite theoretical biases, but with more or less sympathy toward the theory that affection may be accounted for as meaning. Observer F, assistant professor of psychology, stated that he had always held that affections were simple and ultimate psychological processes, capable, except in clearness, of attributive description and coordinate with sensations and

[6] As, it would seem, could Titchener, *Philos. Stud.*, 1902, 20, 382-406; S. P. Hayes, *Amer. J. Psychol.*, 1906, 17, 358-393; W. S. Foster and K. Roese, *ibid.*, 1916, 27, 157-170.

images. Observer P, a graduate student of three years training, was less interested in the general theoretical consideration of affection, but stated that he believed in the possibility of rendering reports upon feeling as independent mental elements. The protocols of Observer M are not considered in this paper for the reason that her lack of introspective training at the time of the experiment leaves her reports equivocal with respect to the point at issue. All the *O*'s, it may be said, however, insisted that they approached the experiment with an open mind, and that their sole motive was to make accurate and detailed descriptions and analyses of the experiences observed under these particular experimental conditions.

Quantitative Summary of Introspections

The absolute frequencies of the occurrence of various affective items in the protocols are presented in Table I, which thus constitutes a rough summary of the introspections and of the individual differences of report among *O*'s. In general, it may be said of the table, the first items refer to the two stimuli compared: "2 P" (=two pleasantnesses) means that pleasantness was reported in connection with each stimulus, "P and U" (=pleasantness and unpleasantness) means that one stimulus was pleasant and the other unpleasant; "1 P" (=one pleasantness) means that one stimulus was pleasant and that there was no affection connected with the other stimulus; and so on. The item "affective or aesthetic characterizations" covers reports of the stimuli as "nice," "good," *etc.*

TABLE I

ABSOLUTE FREQUENCIES OF INTROSPECTIVE REPORTS AS CHARACTERIZED BY THE AFFECTIVE CONTENT INDICATED. P = PLEASANTNESS; U = UNPLEASANTNESS

Observer	B	D	F	P
2 P	12	4	2	1
P and U, or P and not-P	3	0	1	9
2 U	2	1	2	1
1 P	9	0	3	16
1 U	1	3	16	6
Affective or aesthetic characterization	6	30	7	5
No P or U	1	0	17	3
P or U not connected with stimulus	3	1	4	3
Total number of introspections, excluding duplications	31	36	34	39

Summary of Introspections

Observer B. The *Aufgabe* was seldom focal for B in the foreperiod. He was only vaguely aware of the instructions in terms of rigid bodily position and the fixation of attention upon the screen.

The process of comparing varied for him from time to time. As a rule, however, the initial tendency to determine a preference manifested itself with the first perception of one stimulus of the pair and took the form of an incipient absolute judgment of that stimulus; a judgment which never came immediately, but was always preceded by organic and kinaesthetic processes of a certain pattern, accompanying the visual perception and forming in turn the basis of the judgment. B's attention would then shift to the other stimulus and the same procedure would be repeated. During this period he would form a tentative, preferential judgment. The reaction and the announcement of the final judgment usually came with a second perception of one of the alternatives. In cases of difficult judgment the processes were more complex and involved. B would fixate now one and then the other stimulus several times in quick succession, with consciousness dominated by a general emotional stir-up that would seemingly delay the usual organic "building-up-process-for-pleasantness" (see below). In judgment of this kind, the decision was generally accompanied by awareness of the *Aufgabe*.

B's affective judgment was mediate throughout. He never observed pleasantness and unpleasantness as existential mental processes. He reported them as conscious attitudes or "meanings," carried by complexes of organic and kinaesthetic sensations or images. In order to declare a preference, therefore, it was always necessary for him to have this sensory mediator, and he was thus always in search of some such cue. (In the writer's previous paper he has characterized this attitude as a sensorial attitude.[7])

In the early stages of the experiment, B was uncertain of the proper cue for affection. At first he sought to base his judgments of pleasantness upon an *Eindringlichkeit* which consisted of the bare perception of the stimulus with high degree of clearness, intensity and distinctness (*Schärfe*) *plus* the kinaesthetic response of fixation and head-movement toward the stimulus.

[7] *Op. cit.*, p. 88.

"Eye-movement went back to right stimulus and the eye stuck there, as pattern of eye-kinaesthesis, which was quite intense and carried the meaning: 'Striking color.' Then at once automatically throat-kinaesthesis, which meant incipient judgment for the right" (Nov. 18, 1919). "It was *eindringlich, i.e.,* a kinaesthetic context of receptivity. This kinaesthetic context was mostly eye-kinaesethesis, but had a little bodily adjustment in it too. . . . I do not know if anything carried the *P* or was the *P*. It may be that part of the eye-kinaesthesis in the *Eindringlichkeit*-context was or meant *P*, but I am not sure" (Nov. 26, 1919).

In more advanced stages, the criterion of pleasantness changed from *Eindringlichkeit* to what he termed a "building-up-process-for-pleasantness," which consisted of a suffused organic "glow," in quality something like the organic ache of stretching a fatigued muscle:

"Fixation of yellow with yellow both distinct and clear, and an organic attitude (organic sensations in trunk welling up, like a very mild 'affection' in the popular meaning of that term) that I might feel for a pretty tone or a little child. In content simply a pressure-pain glow (pleasant pain, of course) in the stomachic region or just above. Not unlike appetite in sensory nature; totally different in meaning. . . . When the glow got to a given pitch, it meant that the judgment was formed" (Dec. 11, 1919). "I am sure that the thing that builds up *P* is something that is sensory in nature and belongs to the tactual-kinaesthetic-organic groups of qualities. It is very weak and never very clear. I can scarcely localize it, yet I am sure it is in 'me' and not in my legs and arms. I should say it was above the abdomen and partly in the trunk, but that it also involves the facial muscles. Verbally I should label it a *glow,* perhaps a warm glow; but I do not think the sensation warmth actually figures. The phrase is intended to convey the meaning rather than the content. As sensory content I take the thing to belong to the class of pleasant organic pressure-aches, in which the pleasant ache of stretching fatigued muscles belongs. But it is nowise so distinct as the stretching complex; it is just a tiny, little, obscure, glowing tingle, as much painy as a bit of food going into the stomach under one of Carlson's appetites, and by no means so definitely localized" (Dec. 20, 1919).

Although he found exact localization impossible (*vide supra*), at times he reported this organic complex in chest and face, and at other times in the region of stomach and trunk. The 'building-up' process was slow at the very first; later it became very rapid, and still later its contents began to drop out of consciousness. In the final stage, it degenerated to a mere kinaesthesis of adjustment toward the stimulus wherein it was differentiated from '*Eindringlichkeit*' only by presence of a limited amount of "glow" or a trace of the kinaesthesis of a smile. It was at this stage of the experiment that B remarked that mental content might not be essential to the making of a preferential judgment.

"The attitude of preference developed for the right. It is a bare skeleton of the old 'building-up process' now. I look at the stimulus and turn (in imagery, I suppose) toward it, and sustain this attitude. The whole thing came in this time not immediately, but it built up very rapidly. When it had got built up, it was absolutely nothing more than a kinaesthesis of adjustment toward the stimulus, and it meant: 'I prefer that'" (Jan. 20, 1920). "There is a bit of process to the *P*, but less than there used to be; *i.e.*, in perceiving the circle there was a hint of a glow; conscious attitude, perhaps a trace of the kinaesthesis of a smile. Most of the *P*, however, was sheer meaning; *i.e.*, the circle was a pleasant circle, I knew it for a pleasant circle. My guess is that I am reacting to the meaning of *P*, that the content that carries the *P* is not essential to it, but generally exists" (Jan. 27, 1920).

Unpleasant experiences were infrequent with B and he furnished no complete description of a carrier of unpleasantness. One of his reports states:

"I can't describe *U*. I think it is some sort of organic pattern in my body. I actually believe that it is as much like the dull pressure-aches of nausea as *P* is like the bright contacts of tickle; but it is a far cry from *P* and *U* to these simple sensory things" (Dec. 20, 1919).

Observer D. Throughout the foreperiod, D maintained a relaxed bodily position. She had a vague consciousness of the *Aufgabe* to judge and to introspect carried in terms of kinaesthesis in throat and neck and relatively intense strains of the eyes from fixation upon the exposure apparatus.

Her consciousness during the main period consisted chiefly of visual and kinaesthetic-organic experiences. The initial act of preferring for her usually took the form of an attitude of incipient acceptance or rejection of one or the other stimulus, accompanied on many occasions by verbal (imaginal or actually innervated) characterizations of the stimuli. In all other respects her procedure in comparing was similar to B's.

D's attitude of acceptance consisted mainly of kinaesthesis in the head (an incipient nod) and an organic "glow" (chiefly circulatory and respiratory), or of general bodily relaxations and such vocimotor processes as: "That's a nice color;" "rather nice;" "it's a nice shape;" *etc.*

"When the red was fixated, there was a pleasant glow, accompanied by imagery in vocimotor terms: 'That's a lovely red.' I am retrospectively aware that there was a brief arrest of respiration, followed by a deep inspiration which carried the meaning of 'general satisfaction'" (Nov. 13, 1919). "My eyes followed the outline of the figure counter-clockwise from lower right-hand corner. At the same time there was, besides the eye-movement-kinaesthesis, a slightly changed bodily attitude, especially involving kinaesthesis from holding head in particular way. It meant: 'That's a nice compact figure'" (Nov. 18, 1919). "There followed a particular kinaesthetic and organic attitude that meant my affective reaction to this particular stimulus: 'Rather nice'

is what this bodily attitude meant. . . . The consciousness that conveyed the preference was a very vague affair, partly respiratory (as slight inhibition of breathing), partly kinaesthesis of the head (an incipient nod), partly the very turning of the eyes to the accepted stimulus" (Dec. 9, 1919). "There was some eye-kinaesthesis as my eyes followed the outline of the figure, and there was an 'evaluating consciousness' which consisted chiefly of mild but 'pleasantly toned' organic sensations (chiefly respiratory and circulatory) and a bit of throat-kinaesthesis that carried the meaning: 'It's a nice shape'" (March 9, 1920).

The attitude of rejection was not fully described. It seems to have been essentially a complex of kinaesthesis in the region of the diaphragm (constrained breathing, a feeling of tightness), throat-kinaesthesis (similar in quality to nausea), and the accompanying verbomotor processes meaning: "That's not so nice;" "dislike;" *etc.*

"Next came a kinaesthetic set which carried the meaning: 'That's not so nice a figure; it takes too much eye-movement to see it clearly'" (Nov. 18, 1919). "Then followed an attitude of incipient rejection of this stimulus in terms of kinaesthesis in diaphragm region, *i.e.*, constrained breathing; and a bit of throat-kinaesthesis that was related to nausea" (Jan. 8, 1920).

Like B, D found no affective elements as distinguished from sensations. *P* and *U* to her were simply conceptual terms, embracing, among other things, affective characterizations such as "nice," "lovely," "not nice," *etc.*, and designating at the same time an attitude of acceptance or of rejection, which was in content nothing more than a group of kinaesthetic and organic processes. These organic and kinaesthetic sensations appear to be for her a *sine qua non* of affectively toned experiences, for the reason that *P* and *U* are regarded as meanings carried by these organic and kinaesthetic sensations.

Observer F. The foreperiod in the early part of the experiment may be characterized for this *O* as a period of active attention. His eyes were always fixed upon that portion of the screen where the left-hand stimulus would appear. There was a focal visual perception of a limited part of the screen, and intense kinaesthesis in brows, eyes, jaw, neck, chest and abdomen; all of which constituted F's attitudinal response to the *Aufgabe*. Later on this attitude gradually changed into one of passive attention, in which the kinaesthetic and organic processes were very much less marked than before and the visual perception of the screen was alone focal.

In the initial stages of practice, the main period was especially rich in affective and kinaesthetic experience, although the clearest part of consciousness was always the visual perception of the stimuli. In these stages F, like B, made a tentative, absolute judgment of *P* and *U* separately upon each

of the alternatives before determining his preference. These judgments were almost invariably accompanied either by some kinaesthetic-organic adjustment (a nod, strains in the eyes, kinaesthesis in nose and upper lip, or the like) or by certain imaginal or actually innervated verbal expressions, such as: "not so good;" "blue, fine;" *etc.*

"When the screen was raised I was aware of a rapid focal visual perception of the right-hand form. Great *P* raising rapidly to an intense maximum. Verbomotor processes: 'Oh, that one' accompanied by motor image of drawing my hands apart; left hand upward and right hand downward" (Nov. 13, 1919). "Very focal and clear perception of it (stimulus) with rapidly appearing *P*, which rapidly increased in intensity. Verbomotor 'Blue, fine.' Rapid relaxation of bodily strains, deep inhalation and closing of the eyes" (Nov. 13, 1919). "Non-focal kinaesthetic sensations (actually innervated) in nose and upper lip (facial expression of disgust). This meant to me a very unpleasant stimulus" (Nov. 18, 1919).

The termination of preferential judgment usually came while F's eyes were firmly fixing the stimulus and was followed by general bodily relaxations. The difficult judgments were, as for the other *O*'s, characterized by several rapid shifts of fixation and by the presence of unpleasant feeling and strains.

In the later stages the process of comparing became highly mechanized and the final judgment came immediately upon perceiving both stimuli, without even slight *P* or *U* entering consciousness. F's judgment now was reduced to a motor attitude, which consisted of the visual fixation of the stimulus preferred and of his mechanical reaction upon the key.

"This visual perception was terminated by closure of the eyes which was either accompanied by or immediately followed by a very mechanical reaction, both of which were without other antecedent so far as I am aware. . . . I had no affective processes in this experiment whatever" (Jan. 29, 1920). "This visual perception was terminated by a series of processes which took place in the following order. (1) Absolutely mechanical and automatic reaction on the key without other conscious antecedent. (2) Closure of the eyes. (3) Rapid exhalation. (4) Relaxation of the widespread bodily and muscular strains of the foreperiod. . . . I am sure there were no affective processes in the entire experience" (Feb. 12, 1920). "Almost at once upon the beginning of the visual perception I found that I was reacting on the key in a perfectly mechanical fashion and without other antecedent" (March 4, 1920).

F reported *P* and *U* as if they were existential mental stuff. They were always accompanied by organic or kinaesthetic processes of seme sort, but F never saw any connection between these processes and the *P* and *U*. He habitually noted the intensity and the temporal course of a given affection: "Very focal and clear perception of it [stimulus] with rapidly

appearing *P* which rapidly increased in intensity;" "relaxation of general bodily strains and rapid decrease in the intensity of *U*." On the other hand there is nothing in F's reports that insists unequivocally upon the independent status of affection; most of his statements could have been made of conscious attitudes. His judgments, moreover, appear to have been conditioned directly upon the stimuli, especially in the final stages of the experiment, and not to have been consciously mediated by the reported *P* and *U*. (*Cf.* quotations above.)

Observer P. P's mechanism of comparing was similar to the other *O*'s. The foreperiod was characterized for him by the 'attitudinal set' or '*Einstellung*' for carrying out the instructions. This set consisted of muscular strains about neck and shoulders, and organic sensations about lower oesophageal and upper stomachic regions. A general tenseness of body remained constant, but non-focal, until the appearance of the stimuli.

With exposure P fixed his attention alternately upon the two stimuli, evaluating each of them in its turn. Rapid, telescoped, verbal characterizations at times would accompany the perceptions: "That red's good, but orange not bad;" "triangle," *etc.* At the beginning of the evaluating process the non-focal tenseness of the foreperiod would increase momentarily in intensity, and then gradually break down. There would then be a general 'welling up' of organic sensations or an organic 'glow' in the region of stomach and oesophagus, accompanied by *P* or *U* as the case might be, and issuing in the formation of the preferential judgment. When judgment was difficult, the period of evaluation would be much prolonged and several shifts of fixation would take place. In these cases consciousness was dominated by unpleasantly toned organic processes, and the decision occurred only after reappearance of the awareness of the *Aufgabe*.

"This [perception of the form at right] was followed by rapid welling up of acute, intense, organic sensations, localized in oesophagus and stomach, rather intense *U*. Increase in the bodily strains, especially eye-kinaesthesis and in skull" (Nov. 15, 1919). "Focal visual perception of the stimulus at the right and very non-focal visual perception of the stimulus at the left and very non-focal, indistinct, vocimotor-auditory imagery of 'red,' accompanied by slight *P*. . . . Then stimulus at right dropped out to a very non-focal level. The stimulus at left then again became focal. This was accompanied by the slight organic sensations localized in the region of stomach. Then reaction to the 'red-consciousness'" (Dec. 23, 1919). "Fairly focal visual perception of the stimulus, accompanied immediately after by the image (visual) of the left; then *U*. This was accompanied by organic sensations around stomach and oesophagus, a slight increase

TABLE II

OCCURRENCE OF VARIOUS DATA OF CONSCIOUSNESS (MEANINGS, PROCESSES) AT VARIOUS STAGES OF THE EXPERIMENTAL PROCEDURE. 4 OBSERVERS. P = PLEASANTNESS; U = UNPLEASANTNESS

Obs.	Datum	Initial Stage	Second Stage	Third Stage	Final Stage
	P and U as:	Psychological meaning	Psychological meaning	Degenerated psychological meaning	Practically biological meaning
B	Process-basis of P and U.	Eindringlichkeit = P. Organic and kinaesthetic complexes = P.	Building - up - process (organic) = P. Organic and kinaesthetic complexes = P.	A trace, merely, of building - up - process (organic) = P.	P = stimulus itself. Rarely incipient tendency for building-up-process.
	Genetic decay of process-basis.	Rich and well-marked.	Rich and well-marked.	Partial decay of organic process. Rarely bodily attitude only.	Almost complete decay of organic processes. Usually bodily attitude only.
	P and U as:	Psychological meaning		Practically biological meaning	
D	Process-basis of P and U.	Attitude of acceptance (organic and kinaesthetic) = P. Attitude of rejection (organic and kinaesthetic) = U.		Vestigial remains of attitudes of acceptance and rejection.	
	Genetic decay of process-basis.	Rich and well-marked.		Partial but incomplete decay; occasional organic and kinaesthetic processes.	
	P and U as:	Existential mental processes, either unique elements or conscious attitudes		Preference = biological meaning No longer P and U as mental process	
F	Concomitant relevant sensory processes.	Organic and kinaesthetic processes.		Bodily attitude of fixation of stimulus, but no prominent organic or kinaesthetic processes.	
	Genetic decay of relevant processes.	Rich and well-marked.		Almost complete decay of affective and organic processes.	
	P and U as:	Existential mental processes, either unique elements or conscious attitudes			
P	Concomitant relevant sensory processes.	Organic and kinaesthetic processes.			
	Genetic decay of relevant processes.	Rich and well-marked; practically no decay.			

in muscular tension (general, non-localizable) and by kinaesthesis about eyebrows" (Jan. 21, 1920). "Rapid welling up of intense organics about the stomach and oesophagus, accompanied by *P*" (Jan. 31, 1920). [Difficult case]: "Non-focal rapid vocimotor imagery: 'Huh, neither' followed by very rapid shift of fixation from right to left and back again (4 or 5 times), accompanied by organic sensations. Then slight awareness of *Aufgabe* and vocimotor imagery: 'Oh, left is a little better'" (Nov. 18, 1919).

In the advanced stages P's judgment was mechanized and was made at times without presence of reportable *P* or *U*.

Like F, P reported *P* and *U* as if they were mental processes distinct from sensations. He noted their intensities and durations in making comparisons. The organic and kinaesthetic sensations, which were concomitant with affections, he almost always localized in the region of the oesophagus and of the stomach. He gave no clear-cut descriptions of these sensory processes. It seems that the organic glow, usually concurrent with *P*, was of a milder, more diffused nature and thus differentiated from the acute, intense, straining quality that appeared closely associated with *U*.

General Summary

Table II constitutes a summary of the protocols fractionated into successive genetic stages for those *O*'s for whom marked serial changes of consciousness appeared. The table gives for B and D the nature of the meanings of *P* and *U*, the process-bases of these meanings, and a characterization of the extent of the decay of the sensory content. In accordance with Titchener's context-theory of meaning,[8] the table uses the term "psychological meaning" for a meaning that is reportable both as such and in terms of its sensory carrier, and "biological meaning" for a meaning reportable as such that occurs with little or no observable sensory basis. F and P, who imply the independent elementary status of feeling, are summarized in the table by a statement of concomitant organic processes and a characterization of the decay of the total conscious affective experience.

The question of the mental status of the affective process comes up as an issue between observers. B and D were emphatic in their denial of *P* and *U* as directly observable, nonsensory, unique, mental processes, but reported them consistently as unique meanings, carried by complexes of organic

[8] For general statements of the context-theory of meaning, see E. B. Titchener, *Thought-Processes*, 1909, 175 ff.; *Text-book of Psychology*, 1910, 367 ff.; H. P. Weld, Meaning and processes as distinguished by the reaction method, *Titchener Studies in Psychology*, 1917, 181 ff.

and kinaesthetic processes. In any case observable processes were to be expected, since meanings in the early stages of their life history can be expressed in terms of existential sensory process. The issue hinges therefore upon the determination of the sphere in which *P* and *U* are unique and possess independent status, the locatio of this sphere in the realm of observable mental process. Unfortunately the question is one that we cannot finally determine here; we must content ourselves merely with an additional contribution. B called *P* an "attitude-for-pleasantness" and D an "attitude of acceptance." The organic and kinaesthetic bases of these attitudes were (especially for B) very vivid in the early consciousnesses; they underwent, however, the usual degeneration to which the sensory carriers of meaning are subject, and as the experiment progressed they became finally illusive tags of sensation or disappeared altogether. In other words, "psychological meaning" became "biological." In contrast to B and D, F and P reported *P* and *U* as if they were existential mental processes, although they found no occasion especially to assert their existentiality. It is not to be expected that an *O* will especially assert the obvious, and it is thus a reasonable interpretation of their reports to infer that the process-status of feeling was obvious to them and that a present feeling required therefore nothing further than its mention. Nevertheless, when the reports of F and P are taken in the light of the scepticism of B and D, we may perhaps wonder whether the readiness with which *P* and *U* are spoken of as such is a readiness that comes from their independent existential status or the readiness with which conscious attitudes are reportable as mental units. The latter interpretation is supported by the course of F's consciousness (Table II), in which a degeneration of the affective experience was covariant with the degeneration of the concomitant organic and kinaesthetic sensory content.

Taken all in all, our findings may be summarized with respect to the particular point at issue as follows:

1. *P* and *U* may be meanings for any observer; for B and D always; for F consistently at one stage; for P occasionally.

2. Evidence for *P* and *U* as nonsensory existential processes is weaker than evidence for them as sensory mental processes. B and D explicitly describe them as sensory; F and P do not explicitly describe them but merely name them. There is no statement in any introspection that could not have been made of a conscious attitude.

3. Organic sensory content is the *sine qua non* of *P* and *U*, except in advanced stages of degeneration; for B and D as carriers of the pleasant and unpleasant meanings; for F as concomitant or carrier; for P as concomitant.

4. Conscious *P* and *U* drop away from the affective perception in the course of its decay, thus following the law of perceptual contextual sensations.

5. The loss of *P* and *U* is concomitant with the diminution or disappearance of the sensory organic contents of consciousness.

6. The two preceding items of fact indicate that *P* and *U*, whether as sensory or nonsensory, are to be thought of as integral to the total organic complex.

It follows therefore:

(*a*) That *P* and *U* (*of the method of paired comparisons*) are most universally and definitely statable as meanings;

(*b*) that on the side of process *P* and *U* are predominantly sensory; and

(*c*) that there exists a portion of the process-aspect of *P* and *U* which is equivocal in its essential nature (*i.e.*, it is not certain whether it is sensory or nonsensory), but that this equivocal portion of the process-complex follows a law of sensory decay.

The case is by no means conclusive against the status of affection as independent mental process. For one thing it applies only to the method of paired comparisons. It does show, however, that much greater concern needs to be given to the part played by meaning and the illusive carriers of meaning in the consideration of feeling as elicited under this method. Upon the broader extension of these conclusions to the problem of feeling in general the writer is unprepared even to make a surmise. He feels, however, that his results indicate that the method of paired comparisons can no longer be taken as a typical laboratory setting for affections of process-nature, and that the experimental establishment of the process-affection is therefore more than ever in need of an experimental method.

A STUDY IN LOGICAL MEMORY[1]

By Sarah D. Mackay Austin

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Introduction

(1) *The Problem.* The chief aim of this investigation has been to see if certain of the laws established for the recall of nonsense material will hold for sense or logical material as well. The special phase of this problem which has been studied is to determine whether divided repetitions prove more effective than accumulated repetitions in learning material with meaning. The more important subordinate problems which have developed from this larger problem are: the relation of logical learning to rote learning, or memory for ideas as opposed to words; and, in particular, the effect for recall of distributed as compared with accumulated repetitions; and the influence of time (the curve of forgetting) upon the amount and character of the material retained. Although the chief interest in the problem has lain in the quantitative aspect of the process of remembering, careful introspections taken

[1] From the Psychological Laboratory of the University of Michigan.

throughout the experiments show something of the qualitative nature of the problem as well.

(2) *Literature.* Relatively little of the vast amount of literature which has been published on memory and the learning processes has a direct bearing on this problem. Only that work which deals more or less directly with the learning of logical material will be briefly reviewed.

(1) *Learning of (a) Nonsense Material and (b) Logical Material.*

(*a*) The pioneer experiments of Ebbinghaus (3) and Jost (7) in the field of memory were done almost entirely upon material without meaning. Following them have been investigators who have used both material without and material with meaning, as Radossawljewitsch, who used both in his study of forgetting. A number of experiments have been made upon logical material, but usually for verbal recall, not for the memory of ideas as opposed to words. Balaban (1) studied the difference between logical and mechanical memory, using syllables, words, and compound words.

(*b*) Henderson(6) has carried out probably the most complete research with logical material. He had 212 subjects, ranging from school children of the fifth grade to graduate students in college. A typewritten copy of a short prose selection was handed to each subject to be learned in 3 min. At the expiration of that time the subject was required to write as exact a reproduction as possible. The aim of the experiment was to determine the amount and character of the material retained. The subject was then tested two days and four weeks later.

The results of 259 complete papers were scored on the basis of the story, topics, sub-topics, details, and words. All words of the original text that were reproduced in their former contexts were scored full value. Commonplace words, articles, prepositions, and conjunctions were not scored when reproduced out of their context. On the other hand, an unusual word was regarded as remembered even though it appeared in the wrong context. Henderson found that the older students got ideas more than words, and that there was a slight tendency for more advanced students to remember better. He found also that a single reading, concentrated, was nearly as effective for immediate reproduction as 3 min. of study. Details disappeared owing to generalization and there was a tendency to substitute. The details least consonant with the thought were usually forgotten.

Whipple (18, pp. 394-403) refers to the same process of generalization. Referring to Binet and Henri, he says that they found a tendency to express ideas in their own terms, rather than those employed in the original passage. They estimate that memory for connected sentences is approximatly 25 times as good as memory for discrete terms. The general conclusion, we may say, of the investigators using logical material is that ideas are retained, not the original words and that there is a strong tendency to generalize when learning subject-matter with meaning.

(2) *Distributed as opposed to Cumulative Repetitions.*

(*a*) For nonsense material, Ebbinghaus showed the advantage of distributed repetitions in learning. Using the "Saving Method," he

found a saving of more than 30 repetitions in relearning by distributing his repetitions over 3 days. Jost,(7) likewise, found a saving in relearning with repetitions scattered. He used the "Saving" and "Treffer" methods, and found that 30 repetitions at once tested on the following day were very ineffective as compared with the 30 repetitions distributed within limits. He found an advantage in scattered repetitions in both the methods which he used; a saving in relearning, and also a greater number of successes. He concluded that the "Treffer" method was the better of the two.

Perkins'(15) experiments upon nonsense syllables showed also the greater advantage of repeitions distributed within limits. Her main problems were, first, "to discover if there was a limit to the degree of distribution that might be advantageous" and, secondly, to determine, in cases where more than one repetition was made in a single day, "if learning was easier when more than one day was allowed to intervene between the groups of readings, and to discover, if possible, the most favorable period." The results showed that the highest percentage of success was obtained by the use of one reading every third day. She found that, in general, the smaller the number of repetitions used at a sitting, the higher was the percentage of success.

(*b*) For sense material. The most recent experiments in which scattered repetitions are compared with cumulative, and which are very closely related to this problem, are those of Edwards(4). He compared the relative value of 6.5 min. as opposed to 4 plus 2.5 min. when studying a page of history. It was so arranged that the total amount of time was the same for all. Half of each class was a review group, half a non-review group. His subjects were high school and grammar grade pupils. The non-review groups studied 6.5 min. and wrote 12 min. and took an examination from 10 to 12 days later. The review groups studied 4 min., wrote for 12 min., and about 5 or 6 days later reviewed for 2.5 min. It was found that the review groups were better without exception in reproduction. The review groups could do sometimes 50 per cent more than the non-review groups per pupil.

(3) *The Curve of Forgetting of* (*a*) *Nonsense Material,* (*b*) *Logical Material.*

(*a*) In any memory problem, one phase is its relation to the "Curve of Forgetting." Although material with meaning is retained better than nonsense syllables, the same general tendencies are shown in the rate of forgetting. Working with nonsense syllables, Ebbinghaus(3) found that 42% of forgetting took place after 20 min.; 56% after 60 min.; 64% after 9 hrs.; 66% after 24 hrs.; 75% after 6 days; and 79% after 31 days. That is, forgetting took place rapidly at first, and then more slowly.

Radossawljewitsch(17) also found approximately the same results for nonsense material, *i.e.,* a decided loss at first, then a more gradual loss of the syllables that had been learned. His experiments differed from those of Ebbinghaus only in a smaller amount forgotten at first. He found a relatively slow period of forgetting between 6 and 21 days.

(*b*) Although the major portion of his work was done with nonsense syllables, note may be made of one experiment by Ebbinghaus on material with meaning. Even after 22 years, he found a retention of stanzas of poetry learned so that they could be said once, perfectly,

and not seen in the interim. Radossawljewitsch(17) also used material with meaning; a portion of Schiller's *Siege of Troy*. Two stanzas were assigned to his subjects who read them through repeatedly until they could reproduce them twice without error. He found that one-third of the material with meaning was gone in 2 days, one-half in 7 days, while material without meaning was lost much more quickly. With both nonsense and sense material, with these two investigators, the same general "Curve of Forgetting" holds, namely that at first forgetting takes place rapidly and then more gradually.

In connection with the curve of forgetting, Bean(2), in his monograph, presents results from his experiments on forgetting of relatively simple motor acts, sensations with relatively little meaning, and sensori-motor experiences. He says that his own results do not warrant altering the general character of the Ebbinghaus curve of forgetting, nor introducing any other general types of curve. But the rate of forgetting is variable, depending on (1) the degree to which the material had been learned in the first place, (2) the distribution of the repetitions, (3) the kind of material learned, (4) the method by which it is measured, and (5) individual difference in retentiveness.

II. The Method of Experiment

(1) *Subjects.* The following persons served as subjects, Prof. W. B. Pillsbury, Miss Nellie Perkins, Mr. J. Winter, and Mr. H. W. Crane, who performed memory tests during one college year for this experiment. Five other persons served as subjects for a few tests, so few that they had to be discarded. The class in Introductory Psychology furnished material for several tests. The writer served as a subject for a year and a half, and then after an interval of three and a half years continued the experiments for about six months. In the following experiments, these subjects will be designated as follows: Prof. W. B. Pillsbury, Subject I; Mr. Crane, Subject II; Mr. Winter, Subject III; Miss Perkins, Subject IV; Miss MacKay, Subject V.

(2) *Materials.* The so-called logical material was of two kinds for each subject. In each case, the subject chose or was assigned material with which he was relatively unfamiliar but in which he was interested to some extent. Subject I used Organic Chemistry and Russell's *Principles of Mathematics*. Subject II used Embryology (Piersol) and History (a detailed account of the Battle of Gettysburg). Subject III used the same Embryology, and Descriptive Geology. Subject IV used the same Embryology, and a History of South America. Subject V used Economics and a History of Scotland. Embryology was a new subject to those who used it in this experiment, and it presented difficulties in terminology as well as in meaning. The assignments in this were very short.

During the first part of the experiment, Subject V used a text book in *Money and Banking*. Experiments begun after an interval of three and a half years were made with the same text-book and the publications of the National City Bank in addition. The history used in these later experiments was the literature sent by Sir Gilbert Parker to the members of the American Bar Association, Wilson's *Division and Reunion*, Walling's *History of Socialism*, and a *History of French Literature*. In every case, the subjects used books they had never seen before.

Previous experiments in the use of sense material have been limited for the most part to sentences, short paragraphs, or two or three stanzas of poetry. In this experiment, the portions to be learned were much longer, and were always of about the same length. In mapping out the material for the other subjects, as well as for myself, I attempted to have the assignments as nearly equal in length as possible. I laid more emphasis, however, upon the logical place to end the assignment than upon having a uniform number of ideas. The assignments varied, with the exception of those in Embryology, from two pages to four or five. It may be noted, in this connection, that the length of the assignment had little effect upon comparative results.

An assignment covering several pages gives a test of logical memory in its practical aspect, since in school, in college, and in practical life, one is never limited to a mere stanza or paragraph. Such subjects as history and economics, for example, were chosen because they were the sort of subject-matter one generally reads, in addition to being logical material.

When I began working on this problem, after a three and one half year interim, it was necessary for me to use shorter assignments than those which have been described, assignments requiring perhaps 5 to 7 min. to read. This method enabled me to have many tests under way at the same time on different assignments of both history and economics.

Note may be made of one great difficulty which arose and made the number of experiments fewer than they otherwise would have been. The subject-matter for the tests was the consecutive material in a chosen text. Thus two pages, let us say, were chosen for the first assignment, the following two pages for the next assignment, the following two pages for the third, and so forth. If history, for example, had been read five times in one day and was to be tested in two weeks, no further experiments could well be performed in that subject until that test had been made. If a second experiment

had been begun on the following pages before the first had been finished, such confusion in both learning and recall would have resulted that both final results would have been invalidated. It would have been impossible to distinguish one assignment from the other when the time for testing came.

The other subject-matter, embryology, could be introduced in the interim, and experiments made in that, but even then there was a loss of time in the experiments extending over longer intervals. As is evident, this difficulty did not present itself in the short-time tests. Repetitions could be made and the subject-matter tested on the following day; and the questions could be answered two days later and the next test begun. I obviated this difficulty, of the loss of time, in my own later experiments by learning various assignments in several books in both economics and history and having several tests overlap.

(3) *Method of Learning.* The total number of repetitions used throughout the experiment was five, which number was chosen arbitrarily. One repetition consisted in reading the assignment through once very carefully. The subject read his assignments at the same time of day, in so far as was possible. He chose his own method, which was to be uniform throughout the experiment; he could read aloud or not as he wished, slowly or rapidly. After reading the assignment, the subject attempted to drop it completely out of mind. This was very difficult to do at first; since the subject knew it should be remembered, it would be recalled again and again in the effort to have all the subject-matter of the assignment available when the test was to be made. Had this method been continued, it would have introduced a great source of error; some portions might have been reviewed more than others, portions incorrectly recalled might have been gone over until they seemed a part of the context, and there might have been a constant effort to keep the material fresh in mind, particularly if the test were two weeks away. A little practice, however, enabled the subjects to dismiss the assignments completely after reading.

The subject read his assignment as directed; for example, five times in one day, once each day for five days, or three times in one day and two times the next, and wrote all he could recall without any assistance of any kind at the expiration of the stated time. Two days later (this time was also chosen arbitrarily) he answered questions which had been carefully made out, covering as nearly as possible all the ideas in the given assignment. The repetitions were scattered over varying intervals of time, from five repetitions in one day, to

one repetition every fifth day. The extreme time-limits for testing were from two hours to three, four, and six weeks, in one case, each. The majority of the tests, however, lay within the limits (including) two hours and one month. Anything interesting in the way of introspection at the time of reading or of testing was noted by the subject.

(4) *Scoring of Material.* The purpose of this investigation, as has been stated, has been to see how many ideas of any given prose assignment could be retained after certain periods of time, after five careful readings at varying intervals. The original assignment was scored very carefully. I read over the text and counted the number of ideas in this way. One half of a sheet of paper was used to score the ideas in the text, and the other half was used for the test. Usually the first and last words, with abbreviations between, of what I called an idea were written down. To give a concrete example from one of the assignments: "Only that statesman, writer, or sociologist, has the hearing of the public to-day who can bind his proposed reforms into some large far-seeing plan." This was abbreviated as follows:

Socialism

Pages 2 and 3

Text	Test
1. Only states, b. f. plan.	2.
2.	2.
3.	3.

In this way, when it came to scoring a test on the other half of the sheet, the original idea was perfectly obvious. The original assignment, in addition, was always at hand for reference in scoring the tests; for often the idea of the subject was correct, although phrased entirely differently. Just as the original text was scored, so the report of the subject was carefully gone over, the total number of ideas in each found, and the percentage of the ideas recalled in each case was calculated. No scoring was done for words or different parts of speech, the purpose being solely to find the percentage of the whole number of ideas in the original text which could be reproduced.

The scoring was done very closely, making the percentages low, particularly in tests extending over several weeks or more. This was necessary because a high degree of accuracy was obtained in an immediate test, say two or twenty-four hours, and many details were given which would have been forgotten in a longer interval.

The first question which naturally arises in regard to the method of scoring is: how were wrong or partial ideas scored? The answer is that, as a rule, either the subject-matter had been entirely forgotten or was recalled correctly. This was found to be true in general throughout the experiment, although there were a few exceptions. Wrong ideas, dates, or figures would be found occasionally, but comparatively seldom. These were simply disregarded if the idea was correctly recalled. If several partial ideas were found in one test, credit was given for a whole idea, but little scoring of partial ideas was necessary.

All figures in the following tables show the percentage of the material retained, that is, the number of correct ideas the subject retained. The first figure shows the original test, the second, the test made with the aid of the question. Thus, in subsequent discussion, 0—28, means that nothing was remembered by free recall, and 28% of the total number of ideas in the given assignment, with the aid of the questions.

III. Tabulation of Data and Introspections of Subjects

In the following pages the results of each subject will be summarized, and the introspections of interest noted, with particular reference to their bearing upon the problem of imagery or imageless thought. This experiment has shown that, while comparisons of the results of the same individual can be made, none can be made of the results of different individuals.

Table I, Subject I. (1) *Organic Chemistry.* This material required 15 or 20 min. each day to read. It proved interesting, and no pertinent introspections accompanied it. The greater value of divided repetitions is shown as compared with cumulative.

(2) *Mathematics.* These sections were long and difficult. The introspections show: "Not all understood and did not reread anywhere; general impression vague," on the first day. Second day: some mind-wandering. Third day: still rather blind; last part still hazy. Fourth day: "Felt disinclination for the work, one page not appreciated at all in the reading." Again, the introspections taken at the time of reading another assignment: "Read once, room quiet and conditions favorable, pretty well followed. Conditions favorable on following day, and seemed to understand and often to anticipate."

By reference to the table it will be seen that the assignment read two times the first day and three times the second and tested in two weeks gave 0-28 per cent. The notes taken at the time of reading show that it was not understood at the time. The introspections recorded at the time of writing the test show the dependence of recall upon some form of imagery. "At first there is nothing more in the way of recall than a picture of a large page with rather dirty type, with Sec. No. 41 rather low on the left-hand side of the page. As

I hold myself to it, I think that the subject-matter must be of 'implication,' but with no memory of what phase of it may be considered. All is accompanied by unpleasant strain sensations. Nothing more comes. I give it up for a time. Nothing more would come yesterday. Occasionally a stray word would present itself, with the feeling that something else was behind it, but nothing definite would develop, and I could not be sure that the idea came from the right passage."

Summary, Subject I. (1) *Organic Chemistry.* (*a*) Distributed repetitions proved to be more effective than those which followed one another closely, but there seemed to be little difference in value in the spacing of the repetition. That is, material read each day for five days was remembered as well as that read three times the first day and twice the second day. Also material read twice in one day and three times on the following day was recalled as well as that read on alternate days. The results are too few in number to give any definite conclusions, except that when the subject-matter is to be tested in two weeks, repetitions which are scattered are more advantageous. (*b*) The questions brought back more than free recall. (*c*) There was a decrease with time in the amount retained.

(2) *Mathematics.* There were too few results to give any conclusions.

TABLE I

SUBJECT I

Organic Chemistry			Mathematics	
Free Recall	Question Recall	Repetition	Free Recall	Question Recall
		Tested in 2 Weeks		
19.9	19.9	5 times in one day	....	....
25.8	48.6	Daily for 5 days	6.7	14.1
42	52	Alternate days for 5 days	29	40
29.1	55	3 times 1 day, twice 2nd day	24.3	45
40	65	2 times 1 day, three times 2nd day	0	28
		Tested in 10 Days		
43.9	46.1	Daily for 5 days	....	....
		Tested in 7 Days		
		Daily for 5 days	14.8	24.4
		Tested Following Day		
65	74	5 times in 1 day	....	....
72.7	83.3	Daily for 5 days	32.6	36.7

Table II, Subject II. (1) *Embryology.* These assignments were short, owing to difficulty in terminology. The approximate time for reading was from 5 to 6 min. Material read once each day for five days and tested in two weeks gave the highest percentage of any of

the two weeks' tests. One introspection may be noted, showing the subject's dependence on visualization. "I can not remember much about this material. I could not visualize; it has no meaning for me because I can not picture it, or connect it with anything in my experience."

(2) *History.* The history read once every fifth day gives a very low percentage, showing that repetitions so far apart are probably past the limit of advantageous dimension. One introspection may be quoted: "This history I did not feel nearly as sure of as I did the first. I have a general sort of hazy notion; can get but very few facts, and they seem vague and unconnected. During the learning process, I was conscious of the confusion of the details of the material with that of the first. Had I been able to go back over it, I think I should have been able to keep it straight."

The portion read five times in one day and tested on the following gave a test that was an almost exact reproduction of the text, including figures and number of troops. This was very unusual. It may be noted that even after two days, with the additional help of the questions, there was a fall of 5%.

Summary, Subject II.[2] (1) *Embryology.* The results were too few in number for any positive conclusions.

(2) *History.* (*a*) Repetitions distributed within limits (up to one repetition a day for five days) proved most effective for recall when the subject-matter was to be tested in two

TABLE II

SUBJECT II

History			Embryology	
Free Recall	Question Recall	Repetition	Free Recall	Question Recall
		Tested in 2 Weeks		
31.1	40.5	Daily for 5 days	43	75
0	43.4	.....	....	....
....	....	3 times 1 day, twice 2nd day	0	10
....	....	2 times 1 day	....	....
....	....	2 times 1 day, 3 times 2nd day	25	40
4.12	10.9	Once every 5th day	22.2	....
31.9	40	Alternate days for 5 days	15.7	27.7
		Tested in 7 Days		
47.6	54.3	Daily for 5 days	....	....
		Tested Following Day		
98.6	92.3	5 times in 1 day	....	....
70.8	72	Daily for 5 days	65.1	65.1

[2] These subjects, I and II, worked irregularly and the series is not complete. The results, however, show the tendency, confirmed by the more complete series which follow, for the scattered repetitions to have a greater value than the accumulated when the matter is recalled after the longer periods of time, while the two are of approximately the same value for recall after a single day.

weeks. (*b*) For immediate recall the accumulated repetitions proved more valuable than distributed. This, however, was in a single instance only. It shows the tendency, confirmed by the following series of other subjects, that for immediate use the value of the scattered and of the massed repetitions is about equal. (*c*) The questions brought back more than free recall. (*d*) There was a gradual decrease with time of the amount retained.

Table III, Subject III. (1) *Embryology.* The assignment read five times in one day and tested in two weeks gave the following introspection. "I can not recollect distinctly what the material was about. I studied it in succession I think, but I don't remember what it is on. I may be able to answer some of the questions when I see them." It is interesting to note, in relation to this, that the portion read once every fifth day and tested in two weeks gave almost exactly the same percentage and about the same introspection. "Have tried to think just what this section is about, but I can't place it. I have a mass of facts before my mind but I can not pick out the pertinent ones." Repetitions which come too close together or too far apart are of little value. There is utter blankness when trying to recall the subject-matter. Only when some form of imagery is possible does any of the content return.

Another introspection may be noted. "I am completely mystified as to the exact content of the paper I am supposed to reproduce to-day. Not that I do not know some of the facts, but I can not distinguish which they are. I think I shall be able to answer some of the questions without difficulty." When the questions were answered, the following comment was made. "This is a terrific paper, but I can not account for it exactly. Maybe it is due to overwork, or I was not in proper shape when I studied it."

Summary, Subject III. (1) *Embryology.* (*a*) Distributed repetitions are more effective than accumulated, without exception, in the two weeks tests. One reading a day was the most effective distribution. In the material read on alternate days, the initial test with free recall is absent, but the question recall shows the highest percentage of material retained in the two weeks series. (*b*) Repetitions too close together or too far apart are of little value. (*c*) For immediate recall, accumulated repetitions were as valuable as distributed. (*d*) The questions brought back more than free recall. (*e*) There was a decrease with time in the amount retained.

Without exception, with Subject III, distributed repetitions proved more effective for recall after intervals of seven days to two weeks.

(2) *Geology.* The tests were too few in number for a comparative summary. The two points which were shown are: (*a*) Questions brought back more than free recall; (*b*) There was a decrease with time in the amount of the material retained.

TABLE III

SUBJECT III

Geology			Embryology	
Free Recall	Question Recall	Repetition	Free Recall	Question Recall
		Tested in 2 Weeks		
33.3	22.2	Daily for 5 days	26.6	46.6
....	....	5 times in 1 day	0	16.6
13.7	35.5	Alternate days for 5 days	....	57.1
....	....	4 times 1 day, once 2nd day	0	24.2
....	....	Once every 5th day	....	16.3
....	....	3 times one day, twice 2nd day	0	25
		Tested in 10 Days		
13.8	33.3	Daily for 5 days	....	....
35.9	....	.....	....	....
....	....	2 times 1 day, 3 times 2nd day	61.9	....
		Tested in 7 Days		
....	....	Daily for 5 days	38.4	42.1
....	....	Once 1 day, 4 times 2nd day	0	33.3
		Tested Following Day		
61.7	67.6	5 times in 1 day	47.7	63.1
....	....	Daily for 5 days	40	40
40	40	3 times 1 day, twice 2nd day	....	....
		Tested in 2 Hours		
47.2	41.7	Daily for 5 days	85.7	....
48.7	43.6	.....	61.9	71.4
....	....	.....	53.5	50

Table IV, Subject IV. (1) *Embryology.* As has been stated, embryology was entirely new subject-matter; the names and terminology were difficult and the sections, therefore, short. This was the same material used by Subject III. An introspection at the time of testing material read three times in one day and twice the second day is as follows. "I had a perfect picture of all of this at the time of reading. It has completely faded. This was one assignment I felt sure of, but the test shows almost a complete loss of material." After six weeks the questions were answered and 40% was retained, showing that the subject was justified in her feeling of surety.

(2) *History.* I shall quote this subject's introspections in detail, since they give clearly her method of learning and her dependence, for recall, upon a definite image. "In learning the material, I tried to visualize the whole, that is, to put it into an outline form and build up relations between the parts so that one would naturally call up what followed. I also pictured out very accurately every detail. In the recall I found that these two things helped me. It was only after I could not get a mental picture of the details that I could not remember. In recall, I found I was more or less dependent upon my outline. Just as soon as the main facts came to me, the rest fell in order. It seemed to be a continuous process without effort. In many

cases I could nearly get the facts. I could feel them on the fringe and yet they were inhibited. I could see just how they stood on the page, but just what they were I could not get. In several places there was confusion; several things came back, but I did not know their relation to each other or to the whole. I felt that if I could get the name I could fill in the detail. In a few cases whole areas were blank. I could not get a visual picture of the page or fact. The whole mass had fallen away. I knew something was gone, but just what or where I could not tell."

Summary, Subject IV. (1) *Embryology.* (*a*) Distributed repetitions proved more effective than accumulated repetitions, without exception, particularly in the two weeks tests. One repetition each day and one repetition on alternate days were most advantageous. There was little difference in the amount retained when the repetitions were divided in various ways, as three times in one day and twice the second day, or four times in one day and once the second day. Likewise, in the ten and seven day tests, daily repetitions were more valuable than those which were made all in one day or two or three a day. (*b*) For immediate recall, cumulative repetitions were as valuable as distributed. (*c*) Repetitions too far apart, as one repetition every fifth day, as well as those too close together, as five in one day, were of little value. (*d*) The questions brought back more than free recall. (*e*) There was a decrease with time in the amount retained.

(2) *History.* (*a*) Distributed and accumulated repetitions proved to be about equally valuable, for both long and short intervals. This is an exception to the rule which has seemed to hold up to this point. The embryology experiments of Subject IV showed, without exception, the advantage of distributed repetitions for recall after long intervals. One possible explanation for the equal effectiveness of accumulated repetitions in this subject-matter might be that the subject had an unusual ability in remembering historical material. She seemed to be able to reproduce it no matter how it had been read. (*b*) For immediate testing, scattered and massed repetitions were of equal value. (*c*) The questions brought back more than free recall. (*d*) There was a decrease with time in the amount retained.

One review test may be noted, in passing, by Subjects III and IV. They were given on February 26th material they had read the latter part of October and the first of November, and on which they had been tested at that time. The conditions of the review experiment were that they were to reread the original text and test in two hours. The four assignments which were retested in this way showed that, after

several months, the subject-matter first learned with repetitions distributed within the limits of one repetition every fifth day showed a higher percentage of material retained.

TABLE IV

SUBJECT IV

History			Embryology	
Free Recall	Question Recall	Repetition	Free Recall	Question Recall
		Tested in 2 Weeks		
23	42.3	5 times in 1 day	0	27.7
32.5	46.3	Daily for 5 days	7.1	64
45.3	49.4	Daily for 5 days	60	66
23.2	61.3	Alternate days for 5 days	43.4	52.3
33.3	69.8	3 times 1 day, twice 2nd	25	40
16.2	50	Twice 1 day, 3 times 2nd	0	5
....	....	Once every 5th day	0	30
....	....	4 times 1 day, once 2nd day	24.2	24.2
		Tested in 10 Days		
52.6	59.4	Daily for 5 days	36.8	88.4
34.3	62.5	Twice 1 day, 3 times 2nd	19	14.5
		Tested in 7 Days		
59	49	Daily for 5 days	52.5	84.2
54	25.5	Daily for 5 days	52.3	66.6
54	25.5	Daily for 5 days	52.3	66.6
....	....	5 times in 1 day	28.5	45.6
		Tested Following Day		
56.9	62.8	5 times in 1 day	61.1	88.8
82.6	64.2	Daily for 5 days	20	16
52.8	64.3	3 times 1 day, twice 2nd	....	....
		Tested in 2 Hours		
63.4	65.3	Daily for 5 days	78.5	78.5

Table V, Subject V. My own experiments fall into two well-defined groups. The first group consists of tests made in fourteen, ten, seven, four, and two days, and two hours, with the five repetitions divided differently, as three times one day, two times the next, or four times one day and once the following day, etc.

The results of the other subjects, as well as my own, seemed to show the advantage of divided repetitions over longer periods of time. Believing this point to be worthy of further investigation, I undertook the second portion of this experiment. A series of 60 tests was planned. This series was made up of 10 tests of five times in one day, and 10 tests every day for five days, tested the following day, in two weeks, and one month. I made up the questions for myself many weeks, often several months, before I was ready to use the material. I made the sections to be read shorter and, as has been said, I used various books and pamphlets on economics, thereby making it possible

to have many tests under way at the same time. In this way the second series of the 60 tests was carried on more systematically and with less loss of time than the first series.

First Series of the Writer's Experiments

(1) *Economics.* In my introspections I find that, almost without exception, I laid stress upon the necessity of visualization for recall. At first this was more or less unconscious. I did not realize my dependence upon it. One of the comments upon an economics assignment was: "I can visualize where what I don't know is. It is over the page, all of that page, and the top of the next, but I can not get it."

Comment may be made in passing upon one test of material read every day for five days and tested in ten, which gave only 12% in the initial recall. When I made this test, I was ill with tonsilitis. Any state of imperfect physical or mental health or fatigue greatly influences the amount recalled of material which has been learned, and explains departures from the general rule. In this connection there is this introspection; "I simply can't get it at all. This assignment was read five times in succession late at night after a busy day. I know where my last assignment stopped. I am curious to see the questions, for I feel I know some of the material if I could only start, but I have no visualization of any portion of it. I remember exactly where on the page the last assignment stopped, and something of its content, but what follows is an absolute blank." It is interesting to note that the above-mentioned last assignment had been read on five successive days and tested in two weeks. Thus, after several weeks, it was retained better than the later assignment read five times in one day and tested in four.

(2) *History.* The introspections written at the time of reading or of testing the history assignments show the same dependence on visualization for recall. One comment was: "I have lost all visualization here; that is, the continuity is gone, and these isolated facts have returned without any logical connection."

There was one three-weeks test in this group, which gave 3.8% with free recall and later 44% with the questions. The note accompanying the first test was: "I think if I could only start, I could remember it." Evidently this was true, as the questions two days later showed.

After four weeks, material read three times in one day and two times the following day gave very low percentages. Compared with history read in the same way and tested in two weeks, giving 14.6%—25.3% as compared with 9.3%—12.8% in this test, there was but relatively little difference. Little was lost between two weeks and one month.

Several other introspections may be noted which are general in their nature, and summarize in a way the introspections of this portion of the experiment. "After the assignment was read, it was dropped completely out of mind. So completely that often there would be no idea of what the subject-matter was about when the time came to test the material read, for instance, two weeks before. Then, by long continued thinking, the place where the assignment began came into consciousness, that is, on what part of the page. Next, the ideas in the text came, very often many of the same words if the test was an immediate one, but by no means verbatim if the

interval had been a long one since the last reading. When the continuity was lost (and this continuity seemed strangely dependent upon the visualization of the subject-matter), that is, when the logical connection was gone, everything seemed to slip away. Isolated facts might come up, but without their connection. There is nothing quite comparable to the feeling of utter blankness, and of strain as well, in attempting to recall an assignment after a long period of time for which the repetitions had been cumulative. There is present a realiza- that some of the subject-matter is retained, but what, it is impossible to tell, for it was not forcibly enough imprinted to be revived."

Summary of the Writer's First Series of Experiments

The full series of experiments in both economics and history shows the same general tendency throughout. The following summary will, therefore, be based upon the results of the work done in both subjects. For both economics and history, we may say that:

(1) Distributed repetitions proved more valuable than accumulated repetitions for recall, particularly after seven, ten, and fourteen days. One repetition each day and one on alternate days were the most effective. (2) Accumulated repetitions were as effective as scattered for immediate recall. There was no great difference in the amount recalled when the repetitions were divided differently. (3) The questions brought back more than free recall. (4) There was a decrease with time in the amount retained.

TABLE V

SUBJECT V, FIRST SERIES

History			Economics	
Free Recall	Question Recall	Repetition	Free Recall	Question Recall
		Tested in 2 Weeks		
36.5	36.5	Alternate days for 5 repetitions.....	22.2	33.3
14.6	25.3	3 times 1 day, twice 2nd.....	11.7	33.3
0	28.8	Twice 1 day, 3 times 2nd.....	17.4	30
		Tested in 10 Days		
44.8	49	Daily for 5 days.....	12.3	38.4
42.5	....	Daily for 5 days.....	....	....
18	38	3 times 1 day, twice 2nd.....	20	28.5
....	....	3 times 1 day, twice 2nd.....	0	32.4
....	....	Once 1st day, 4 times 2nd.....	28.5	46.4
25	33	5 times in 1 day.....	24.2	36.3

TABLE V—Continued

SUBJECT V, FIRST SERIES

History			Economics	
Free Recall	Question Recall	Repetition	Free Recall	Question Recall
		Tested in 7 Days		
46.8	59.3	Daily for 5 days	54	25.5
....	....	Daily for 5 days	36.1	44.9
5.7	30.7	Once 1st day, 4 times 2nd	32.2	41.9
34.6	38.4	3 times 1 day, twice 2nd	30.4	30.4
35.6	43.8	5 times in 1 day	20	33
		Tested in 4 Days		
46	52	5 times in 1 day	0	21.6
43.3	52.8	Daily for 5 days	47.3	47.3
51.8	62.5	3 times 1 day, twice 2nd	25.6	42.8
48.2	54.2	Once 1st day, 4 times 2nd	39.5	55.8
58.8	64.7	Once 1st day, 4 times 2nd	....	....
21	40	Twice 1 day, 3 times 2nd	38.4	53.8
		Tested Following Day		
52.8	65.5	3 times 1 day, twice 2nd	67.5	52.5
....	....	3 times 1 day, twice 2nd	40	46.6
47.1	54.5	Once 1st day, four times 2nd	52.5	56
52.1	52.1	Once 1st day, four times 2nd	....	....
....	....	Daily for 3 days, 1 day omitted and 2 succeeding days	41.9	54.4
....	....	Every alternate day for 5 repetitions, 2 days between last 2 readings	46.1	46.1
54	56	4 times 1 day, once 2nd		
58.1	66.6	Alternate days for 5 repetitions	47.5	55
....	....	Alternate days for 5 repetitions	52.9	52.9
58.8	68.8	Twice 1st, twice 2nd, once 3rd	....	....
		Tested in 2 Hours		
76.7	76.7	5 times in 1 day	67.7	67.7
70.3	79.2	Daily for 5 days	56.9	54.9
64.5	56.1	3 times 1 day, twice 2nd	65.7	60
		Tested in 3 Weeks		
3.8	44.2	Twice 1 day, 3 times 2nd	....	....
		Tested in 4 Weeks		
9.3	12.8	3 times 1 day, twice 2nd	....	....

Second Series of the Writer's Experiments

The particular noteworthy point which had been developed thus far was the greater value of distributed repetitions in learning, when the subject-matter was to be tested after a long interval. The following experiments were performed in an attempt to develop this hypothesis into a theory. A series

of ten experiments each, of five repetitions in one day and five repetitions, one each day, tested on the following day, in two weeks, and one month was performed, making a total of 60 tests (or 120 in all, including the answers to the questions).

The results show a marked degree of consistency. All the figures which I shall use to compare one series with another will be averages of each series of ten experiments. For example, 11.49%—22.41% means that the average amount recalled for the ten tests, five in history and five in economics, for free recall was 11.49%, and with the aid of the questions, 22.41%.

(1) The first comparison of averages in this series will be that of material read five times in one day, and of material read each day for five days, both tested in one month.

Read	Tested	Average (10 tests)	Mean Variation
5 times in 1 day	1 month	11.49—22.41	4.8 — 6.48
Daily for 5 days	1 month	30.59—41.66	9.41—10.84

That is, almost three times as much was recalled after the end of a month, when the repetitions were divided. Almost twice as much was recalled with the aid of the questions when the repetitions were divided.

(2) The second comparison is as follows. The time of testing in this case was two weeks after the last reading.

Read	Tested	Average (10 tests)	Mean Variation
5 times in one day	2 weeks	13.13—30.2	10.21—10.57
Daily for 5 days	2 weeks	37.26—49.09	11.24—11.95

The relation is much the same as in the tests of one month; almost three times as much was retained when the repetitions were scattered. There is not so much difference in the amount retained with the questions as in the preceding case, although appreciably more was recalled with the questions when the repetitions had been scattered.

(3) The third comparison of this series is of the averages of subject matter tested the day after reading.

Read	Tested	Average (10 tests)	Mean Variation
5 times in 1 day	Next day	66 —69.5	10.62— 8.87
Daily for 5 days	Next day	64.4—70.6	10.57—10.19

For immediate recall, cumulative repetitions proved as effective as distributed. All of the subjects in this experiment have shown the same result, indicating the tendency for accumulated repetitions to be as effective as those which are scattered. There are always many factors which might influence a single test, and make variable the amount retained. This series is significant in that it represents averages, and therefore confirms the point that repetitions which follow one another closely are as valuable as those which are distributed, if material is to be utilized at once.

The introspections of this series have the same tenor as those of the former series and of the other subjects of the experiment. I shall quote only a few. Of the history assignments tested in one month we find: "I can see where the assignment is on the page even to the paragraphs; I know that if I can ever start, some facts will come back to me. I have no idea as to the words the author used; everything is general. I know just where a long succession of facts in the text is; if I only had the right cue to set me off, I am sure I could reproduce some of them. But it is all exasperatingly faint." I quote this because it is typical of many of the introspections written at the time of testing assignments read a month before. Again, "I have entirely lost all the wording of this; the sentences were rather long as I recall them, and only a few of the main ideas come, clothed in my own words."

Another comment was: "I have a general idea of the whole, but the several Russian names throughout the portion confused me. While learning it, I remember thinking, 'Now if I can't recall those names, I won't recall any of it, and I am afraid I can't recall them.'" It seems to have worked out so.

A general observation of the material read five times in one day and tested in one month is: "Invariably there is a decided vagueness, and if anything is recalled it is only in the most general terms. I have a visual picture of the whole assignment, and although I can see where it begins and ends, I have but little idea of the content."

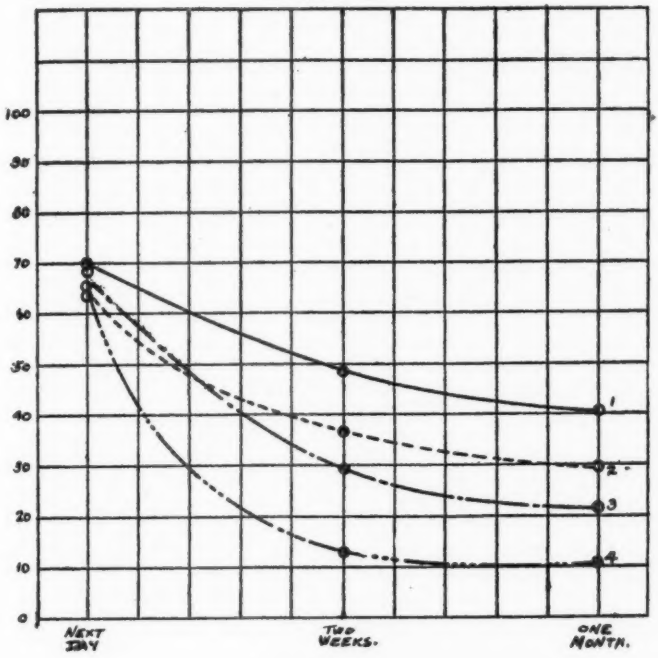
Similarly, of a test five times in one day and tested in two weeks, we find: "I can see this whole paragraph but I know absolutely nothing in it." Again, "I know that this assignment began on the right-hand side of the page about the middle, but I simply cannot recall anything." Very careful and full introspections were taken, but it is sufficient to quote these which I have chosen as typical of them all.

I do not know how I knew what the general idea was when I could not recall it. It is significant that I had to have, first of all, the visual picture of the appearance of the page for any recall; then came the feeling that I knew something of the subject-matter in the text. Yet I could not bridge the gap between my concrete image of the printed page, which I had once known, and the general idea of its content.

The Curve of Forgetting

The writer's second series of experiments contains data sufficient to show the influence of time upon the amount of material retained.

CURVE I.



LEGEND

1. MATERIAL READ EVERY DAY FOR FIVE DAYS QUESTION RECALL ——————
2. MATERIAL READ EVERY DAY FOR FIVE DAYS FREE RECALL - - - - - - - - - - - -
3. MATERIAL READ FIVE TIMES IN ONE DAY QUESTION RECALL: —— • —— • —— •
4. MATERIAL READ FIVE TIMES IN ONE DAY FREE RECALL —— ··· —— ··· ——

SUBJECT MATTER READ FIVE TIMES IN ONE DAY

Tested	Free Recall	Question Recall
Following day.....	66 % retained	69. 5% retained
In 2 weeks.....	13. 3% "	30.12% "
In 1 month.....	11.49% "	22.41% "

SUBJECT MATTER READ FIVE TIMES IN ONE DAY

Tested	Free Recall	Question Recall
Following day.....	64.41% retained	70.59% retained
In 2 weeks.....	37.26% "	49.69% "
In 1 month.....	30 % "	41.66% "

The accompanying curve shows this result graphically. It conforms with the work done on the Curve of Forgetting. Forgetting occurred rapidly at first, and then relatively little was forgotten in the period from two weeks to one month. Forgetting occurred much more rapidly when the material had been learned with the repetitions massed, for the first two weeks, but after that the loss was slight. There was a much greater drop in the curve when the repetitions were accumulated. No matter in what way the material was learned, the loss in the first two weeks was very rapid. It is for retention after longer intervals that the value of divided repetitions is particularly noticeable. We may say, then, that for these experiments, Ebbinghaus' Curve of Forgetting applies to sense as well as to nonsense material.

Class Experiments.—The writer made some experiments on a class in Introductory Psychology. The results of four written lessons show the same general tendency we have found throughout; namely, that, for immediate recall, readings following one another closely are as effective as readings which are scattered. The method used was this. The instructor assigned a certain number of pages in the text-book in Elementary Psychology. He asked one half of the class, in preparation for their twenty minute written lesson, to read the assignment three times in succession, the other half to read it on three alternate days. One hundred eighty-five papers written by students who conformed to the instructions were obtained, despite the failure of some of the students to follow directions accurately.

The ideas in the text were scored as has been described, and the average number of ideas that could be reproduced for each group was found.

TABLE V

SUBJECT V, SECOND SERIES

History			Economics	
Free Recall	Question Recall	Repetition	Free Recall	Question Recall
		Tested in 1 Month		
10.5	26.3	5 times in 1 day	18.7	31.2
0	15	5 times in 1 day	0	16.6
10.5	15.7	5 times in 1 day	11.7	23.5
13.6	27.2	5 times in 1 day	9	27.2
21	31.5	5 times in 1 day	9.9	9.9
Average for free recall for both subjects 11.49, M. V. 4.8				
Average for question recall for both subjects 22.41, M. V. 6.48				
40.9	45.4	Daily for 5 days	41.1	58.8
18.7	31.2	Daily for 5 days	26.6	33.3
26.3	31.5	Daily for 5 days	20	33
22.2	38.8	Daily for 5 days	22.7	31.8
31.2	37.5	Daily for 5 days	56.2	75
Average for free recall for both subjects, 30.59, M. V. 9.41.				
Average for question recall for both subjects, 41.66, M. V. 10.84.				
		Tested in 2 Weeks		
9	32.7	5 times in 1 day	0	20.5
0	20	5 times in 1 day	0	7.1
31.5	42.1	5 times in 1 day	24.4	44.4
22.2	33.3	5 times in 1 day	26.6	40
0	20	5 times in 1 day	17.6	41.1
Average for free recall for both subjects, 13.13, M. V. 10.29.				
Average for question recall for both subjects, 30.22, M. V. 10.57.				
20	32.5	Daily for 5 days	40	50
47.3	63.1	Daily for 5 days	64.7	64.7
40	66.6	Daily for 5 days	50	61.1
25	33.3	Daily for 5 days	17.6	23.5
34.7	52.1	Daily for 5 days	33.3	50
Average for free recall for both subjects, 37.26, M. V. 11.24.				
Average for question recall for both subjects, 49.69, M. V. 11.95.				
		Tested Following Day		
55.7	55.7	5 times in 1 day	54.6	62.5
58.4	66	5 times in 1 day	51.3	51.3
90	85	5 times in 1 day	76.4	76.4
73.9	73.9	5 times in 1 day	60	73.3
72.2	83.3	5 times in 1 day	67.7	67.7
Average for free recall for both subjects, 66, M. V. 10.62.				
Average for question recall for both subjects, 69.5, M. V. 8.87.				
53.3	66.6	Daily for 5 days	49.3	49.3
49	63.4	Daily for 5 days	77.7	83.3
70.5	76.4	Daily for 5 days	61.9	66.6
62.5	62.5	Daily for 5 days	94.4	100
57.1	64.2	Daily for 5 days	68.4	73.6
Average for free recall for both subjects, 64.4, M. V. 10.57.				
Average for question recall for both subjects, 70.6, M. V. 10.19.				

"A" is material read three times in succession.
"B" is material read on three alternate days.

The table showing the results from these tests is as follows:

Quiz	No. of Papers	How Read	Average % Retained
I	17	A	34
	15	B	46
II	25	A	45.7
	24	B	45.6
III	34	A	49
	19	B	52.4
IV	32	A	40.2
	19	B	39.3

These results conform with the general tendency which has been shown in the experiments of the subjects tested at length, namely, that readings which are distributed and readings which are accumulated are equally effective for immediate recall.

Brief General Summary of the Results of All the Subjects of the Experiment

(1) Distributed repetitions were more valuable than those which were accumulated when the subject-matter was to be tested in two weeks or one month.

The experiments of Subject I in Organic Chemistry and Subject II in History, though few in number, followed this rule, and, without exception, the Embryology tests of Subject III. (The Geology experiments of Subject III were too few in number to be significant.) The full series of Embryology experiments of Subject IV followed the same law. An exception is to be found in the History experiments of Subject IV, in which accumulated and distributed repetitions were of equal value. The single experiments in the first series of Subject V showed the advantage of scattered repetitions, yet not so strikingly as the second series. In the latter, without any exception, distributed repetitions proved to be very much more valuable, for periods of two weeks and one month. Since the final percentages of this series are based upon averages, not single experiments, we may attach much more importance to them.

(2) For immediate recall (recall after two or twenty-four hours) accumulated repetitions were as valuable as distributed. This was found to be true throughout the experiments for all subjects and for the tests made upon the class in Elementary Psychology.

(3) Repetitions too far apart were as ineffective as those which came too closely together. Although there were relatively few of the tests with repetitions distributed to one every fifth day, the results which were obtained showed that repetitions so far apart were almost valueless.

(4) The questions always brought back more than free recall.

(5) There was a decrease with time in the amount retained.

(6) For all subjects, there was a dependence for recall upon visualization of the text.

IV. Discussion of Results and Summary

(1) *Discussion of Results*

(*a*) *Sources of Error.* The sources of error which form an important factor in such an experiment as this should first be noted. Most important of all, perhaps, is the "personal equation," the individual variation in each subject from day to day, in general health, fatigue, mood, interest and lack of interest. This is an element which cannot be controlled experimentally. A passing mood, or a state of fatigue at the time of learning or of testing, will entirely alter the usual result.

Another source of error which cannot be controlled is the lack of perfect uniformity in the difficulty of the material to be learned. The same books and pamphlets were used throughout the experiment, but as in all subject-matter some pages were easier than others. They had to be taken in succession, otherwise the logical connection would have been lost. Thus if a few pages happened to be easier to remember and the repetitions to be distributed advantageously, an unusually high per cent was retained. This lack of absolute uniformity in the subject-matter was a constant factor, and can only be eliminated by averaging a number of results.

It may be said that knowing the time the test was to be made might introduce a source of error. Whether it was to be made in two hours or in two weeks might, unconsciously, influence the amount of effort expended in learning. Under ideal conditions of experiment, the subject would not know when he was to be tested. Practically, however, it was necessary to map out the work in advance, and the subject was given explicit directions when to read and when to test, for a short series of experiments at a time. In my own later experiments, when I had several tests overlapping, I paid but very little attention, if any, to the time of testing, while I was reading the assignment.

Another criticism, and perhaps a just one, is that I scored the text, test, and made out the questions on my own material. This may appear to have given me an unfair advantage in having seen the subject-matter before beginning to learn it. I do not think, however, that this was the case. The questions on the subject-matter were made out for other subjects as well as for myself, often months before they were to be used. As a rule, I did not even recognize the material when I read it.

Again, I scored all material myself; it was not rechecked by anyone else. I made two checks to see if this criticism was valid, and to see if my method of scoring was consistent. First, I scored again some tests of my first series which had been made a year or more before, and secured practically the same percentages. Secondly, when I began my experiments again, after an interval of over three years, during which time the problem was not touched, not even thought of, I compared the results with those obtained before the work was interrupted, and found a general uniformity. These two checks seem to indicate that the criticism has no vital significance.

(*b*) *Statement of Results and Their Practical Application*

One of the points to be considered first in a discussion of the results is this. All the subjects without exception, some to a far greater degree than others, were dependent upon visualization of the subject-matter for reproduction. There was in no case recall of the logical sense of the subject-matter without some imagery. This is in harmony with Finkenbinder's (5) investigations. He found with 18 subjects using problems of various kinds and of varying degrees of difficulty, as mathematical problems, mechanical puzzles and the like, that the visual image was the first definite figure to appear in 95% of the recalls. In this connection Offner(13) says (in relation to reproduction) that we have the factor of the appearance of the text, having a certain association of words in a spatial scheme.

Since visualization was essential for any reproduction, the results of my experiments agree with those of the investigators who hold that there must be some imaginal factor for recall, as opposed to those observers who hold that imagery is not essential. We can only conjecture as to the dependence upon imagery had some of the subjects for this experiment been of a non-visual type, as the acoustic-motor type, for example.

It is interesting to note, as has been stated before, that there were few errors in the tests which were scored. Gross

errors were seldom made. As a rule, if the ideas in the text were retained, they remained in their correct form. In the immediate tests, the author's style and his actual wording were sometimes reproduced. As the time-intervals for testing increased, however, the important ideas remained and became more generalized, the original wording of the author was lost, and more and more details dropped out. This also coincides with the general results of memory investigations in logical material, particularly with those of Henderson. We also found a tendency to generalize.

One of the most important results of this investigation is to show that for immediate recall accumulated repetitions are as valuable as divided. This harmonizes with general observations. It is a common experience for us to learn something for immediate use; say, to be utilized in a few hours or on the following day. School children learn for immediate reproduction; the more quickly the content is reproduced the more nearly accurate it is. Offner(13) asks: "May the greater value of accumulated repetitions for recall be attributed to our habits of learning, or are our habits of learning due to this?"

Accumulated repetitions make material available for immediate use: in common parlance this is really "cramming." An example in a superlative degree is the actor's cramming or "winging" of a part. "A part taken in the morning can be done perfectly at night; every scene was read before going on, the shape of the part was mechanically fixed in memory, that is, the position of each speech on the paper. Words would be acquired but not always the sense, for there would be no time to think about the context . . . . . One result of these hurried studies was, words would not remain, fading from the memory as rapidly as acquired."[8]

The same principles apply in a lesser degree to "cramming for examinations." Lessons which were not learned at the time they were assigned can be so intensively studied that, for 12 or 24 hours, the greater part of the work of the semester can be retained. It is a common observation that anything so learned vanishes almost immediately.

The facts in practice, then, are that for immediate use a great mass of material can be made available by intensive review or "cramming," a short time before it is to be utilized. tical life, carried on as accurately as possible, showed exactly Our experiments upon subject-matter such as is used in practhe same conditions to be true. We may conclude, therefore,

[8] H. F. Osborn, Rapid Memorizing, *Psych. Rev.*, ix., 1902, 183 f.

that subject-matter which is studied very intensively a short time before it is to be used, both in practical life and in scientific investigation, is soon lost, and very little if any of it can be recalled after a lapse of time.

The second important result of this research is the fact that accumulated repetitions, which are highly effective for immediate recall, are ineffective for recall after several weeks. Our experiments show the very great advantage of divided repetitions when learning is to be tested after longer time intervals, particularly two weeks and longer, after the last reading.

While our experiments cover too few periods of time to give a complete picture or to formulate an equation, our results for all of the subjects of the experiment show that forgetting took place rapidly at first, and then much more slowly. Meumann says that whatever we know twelve or more days after learning presumably stands longer at our disposal. On the other hand, what we can reproduce at a glance or at a single reading, we have for only a short time. Similarly in our experiments, there was a rapid loss at first, while if the subject-matter were retained two weeks there was little additional loss at the end of four weeks. Our few results indicate that the curve of forgetting has the same general form for logical as for nonsense material.

The fact that divided repetitions have an advantage over accumulated has been explained in two distinct ways. The first suggestions were based on the assumption that the distributed repetitions had some advantage for learning inherent in the distribution itself, that there was less fatigue or ennui or other disturbance, when few repetitions were made at one time. A second hypothesis is that the distributed repetitions offer opportunities for some favorable change in the effects of learning, some change that takes place after the learning gives the distributed repetitions an advantage. Each of these general classes offers several subordinate suggestions. The first suggestion of the first class was made by Müller, to the effect that the fatigue from many repetitions in succession would explain the more favorable results from the distributed repetitions. Under Müller's direction Jost made a series of tests in which a number of repetitions of other series were made before each of the repetitions with distributed learnings that should make the total number equal to the number given to the accumulated repetitions. This insured that the subject should be more fatigued or bored during the divided than during the accumulated repetitions. Neverthe-

less the advantage of the divided repetitions persisted. Müller seems to have accepted Jost's results as convincing evidence that his suggestion did not furnish the real explanation.

Another explanation in terms of conditions incident to the nature of the individual has been suggested recently for animals. Animals, too, learn a maze much more quickly with divided than with accumulated repetitions. In testing the phenomenon Lashley (10) confirmed the results of earlier investigators objectively, but attributed the fact to the dislike of being taken away from the food, and to general handling. This was an immediate accompaniment of reaching the food-box in accumulated repetitions, but was not so prominent in the divided repetitions. He is particularly vigorous in his preference for this as opposed to any 'setting' process in the nervous system as an explanation of the advantage for the divided repetitions. Whatever may be the status of this explanation for animal learning, there is no corresponding advantage for distributed repetitions in human learning. As Jost's experiments show, fatigue or ennui is not a sufficient explanation. One factor that might be expected to favor accumulated repetitions may be suggested. This is the evidence of making progress in the learning. As several repetitions are made at one sitting one understands, and feels in consequence a confidence in the ability to repeat, that is pleasant as a sign of progress. To be sure one does not actually know that one is learning. The only direct evidence of that is furnished by the test itself, but confidence of that ability increases with understanding. Were any detrimental influence of accumulated repetitions to exist in man, one would expect it to show itself in Jost's experiment mentioned above, in which the divided repetitions were repeated after a number of repetitions had been made of other series to induce the same state of fatigue.

The more usual explanation of the second type is in terms of the perseveration tendency. This must stand or fall with the interpretation of that term. If we assume with Müller that there is a continuance of the activity of the nervous system for some time, as long as forty-eight hours, and that during that time associations constantly increase in strength, we should have all that is essential for our purposes. In this assumption it is necessary to distinguish what might be called the immediate readiness for response from the persisting tendency to return that is connected with association. This may be made clear if we think of dispositions left in the nervous system as consisting of two parts; one, more temporary, which

gives the nerve tract affected a tendency to continue active for a relatively short time, and for this activity to become conscious whenever there is no other activity present; the other, more permanent, which serves in some way to connect that tract with another involved when the tract excited with it or immediately after it is aroused. This is the ordinary association. The former tendency dies away relatively quickly; the second persists in some degree for years and diminishes relatively slowly. Since the tracts are active in the same way during the perseveration period as during the original action, it may readily be assumed that the associative process or formation of connection between that and the tracts active during the process accompanying and immediately succeeding will be increasing in strength during the period of partial activity. If we grant this assumption, the greater effect of divided repetitions will be due to the fact that after each repetition the activity will continue for a time. With each repetition this activity will start at a maximum and will diminish to zero. When the same number of repetitions is made in immediate succession, the degree of activity of the tissues involved will not be much greater after the series than after each repetition, and its return to zero will be almost as quick as after each of the divided repetitions. What is gained for the divided repetitions as compared with the accumulated is the setting due to the perseveration for each of the groups of repetitions as compared with the single repetition.

The evidence for the existence of perseveration is, first, the general existence of a continuance of the cortical excitations as seen in the memory after-image. It is fair to assume that there is no sharp cessation of this persisting effect: the only question is how long it continues. Müller and Pilzecker, who first made use of the term perseveration, although the phenomenon had been observed by others before, cited the instances of songs that run in the head, the recurrence of vivid images of exciting positions in a game of chess, etc., which last for hours and occasionally days, as compared with the few seconds that the memory after-image has been demonstrated to continue. They also found that certain syllables shown in one experiment were likely to reappear in another experiment soon after, although there were no syllables shown, or other associations, that would suggest them. Foster, who repeated the experiments with the object of deciding whether these cases were sufficiently frequent to justify the assumption that perseveration was a factor to be reckoned with, came to the conclusion that cases of appearance of old syllables were

due to unnoticed associations, or to the fact that they were sounds likely to be made because of their ease or similarity to emotional expressions. It should be noticed that all that he claims is that they might be due to association, not that he was able to trace the associates in each case; and the ease or resemblance to emotional expressions is a matter of opinion. Foster also does not question the persistence of the nervous activity, but only the likelihood that experiences will rise to consciousness soon after their original appearance when not excited through associations. He grants all that is needed for the explanation of our facts.

The indirect evidence is also striking. I mean by indirect evidence in this connection a series of well established facts which can be readily explained if we assume persveration, but which are hard to understand if we deny it. There are three such sets of facts: first, retroactive inhibition; secondly, the rapid forgetting for the first few hours or for two days, followed by a relatively slow decline of the strength of associations, and thirdly the fact we are dealing with, the advantage of divided repetitions. We can understand retroactive inhibition and the related retrograde amnesia of pathological states if we assume that the effects of experiences are at first of relatively little value for the formation of associations that shall lead to their reinstatement, and that, with the passage of time, through the perseveration they become more firmly established. The rapid course of forgetting of meaningless or nonsense material at first might also be explained if there are two parts to the memory process, one due to a quickly disappearing tendency to return, the other to a tendency that diminishes very slowly. The rapid diminution of the likelihood of recall at first would be due to the gradual dying out of the perseveration, while the later part of the curve would represent the more gradual weakening of associative bonds. Finally, we see that the perseverative tendency makes explicable the advantage of divided repetitions. Miss Perkins' results would also give an indication of the length of time the perseveration continues to be strong enough to be detected. Her results show that divisions up to one every third day were more effective than accumulated repetitions, and our present investigation confirms that for sense material. This agrees with the form of the curve of forgetting, which loses its sharp descent at the end of the second day, according to both investigations of the subject. We find, then, an explanation of the three important general facts and of the more detailed

time relations, if we assume that retention is partly due to perseveration, partly due to association.

Closely related to this explanation is Jost's suggestion that the determining advantage of the divided repetitions is to be sought in the fact that older associations are more easily brought to full effect by new repetitions, and also that older associations are loosened less quickly than more recently formed ones even if they have the same strength. This law was established before Müller and Pilzecker formulated their doctrine of perseveration, and so takes no account of the latter. It is easy to see that perseveration constitutes an explanation of the greater persistence and more ready revivability of the older associates. If we assume the two parts or phases of the retention process, perseveration and association, the associations would increase in strength during (and because of) the perseveration process. The tests made soon after learning would give a high percentage of successes by the method of successes, because part of the recall would be due to the perseveration. They would not be so quickly learned to the full, because the association process would not have attained the strength that it would have after the perseveration period has passed, and this alone apparently is of value, or at least is of greatest value, in learning to the point where the series can be repeated without promptings.

It is interesting to note that both in these experiments and in Miss Perkins' the distributions could be greater than were advantageous. We found no effect, or very slight effect, from repetitions made one each fifth day. Miss Perkins found that repetitions one each fourth day were less effective than one each third day. This might emphasize the second part of Jost's first law, that old repetitions are more easily brought to full strength than are new of the same strength. Taken with no other implication this might mean that the testing was of little or no value unless the associative trace was refreshed by a new repetition made soon after the perseveration had ceased. This is compatible with the perseveration theory, but one must admit the fact that the effect of the immediate repetition *plus* the setting is lost unless a new repetition is made within a certain time. The time must be before all effect of the first repetition is lost, and to obtain the best effect it must come when we may assume the perseveration of the original repetition is just disappearing.

Ladd-Woodworth(9) suggest that some of the effect of the division may be due to the more rapid forgetting of errors, which serves to leave the correct responses more prominent

than they are when the repetitions are accumulated. This would explain the more rapid learning of the mechanical operations of men and animals. It would be less important in rote or logical learning of verbal or symbolic material, in which few errors are made, and where the distributed form of learning is more effective even if each repetition is perfectly accurate. There it must be subordinate to some one of the other explanations.

On the whole it seems that there must be some change in the associations that takes place after each group of repetitions which is more favorable to the divided repetitions. In the light of Jost's law this also favors all older formed associations as compared with the new formed. The most likely source of this improvement in the period after each repetition is the perseveration tendency. The associations increase in strength during the perseveration, and if a new repetition is made before its strength has waned too much the effect of the distributions is beneficial.

We also need to explain the most striking general result of this investigation, that divided repetitions are relatively more favorable after a long period than after a short, after a week or a month than after twenty-four hours. Miss Perkins states that she had some evidence to this effect in her series of nonsense syllables. If we compare her results with those obtained by Jost we find that the difference between the published results of the two is not very great. Jost tested after twenty-four hours, Miss Perkins after two weeks. If we compare his results for 3x8 (eight repetitions on each of three days) with her 2x8, and his 6x4 with her 2x4, and his 2x12 with her 2x8, we find that he has 18, 39, and 53% for one observer and 7, 31, and 55% for another as against an average for all observers for Miss Perkins of 9, 25, and 43%. If we average the results of Jost's two observers there is a slightly greater difference in favor of the more distributed repetitions after the longer period, but it is so slight as to be negligible as compared with the differences we have found for sense material. That the difference should exist does not seem open to explanation from the facts of related phenomena, although it harmonizes with the observations of daily life.

An explanation of the fact of the greater effect of distribution after a considerable period than after a short might be found in the mere average time that elapses between learning and test. For if one repeat once a day for five days and test on the day following the last repetition, the average time between learning and test would be four days as compared

with one for the accumulated repetitions. On the other hand, if the test came after two weeks, the average time elapsed would be seventeen days for the distributed as compared with fourteen by the accumulated. The advantages of the distribution might be sufficient to overcome the smaller relative advantage of the long interval and not that of the short interval. There is nothing in the experiments to indicate why the difference should be more apparent with sense than with nonsense material. And the average time between repetition and test would be present for nonsense as well as for sense material. It is evident that other factors are necessary to complete the explanation. Whatever the reason, the increased effect of perseveration or the resulting 'setting' becomes more apparent after the lapse of considerable time.

(2) *Summary*

(1) Divided repetitions, within limits, prove more effective than cumulative repetitions with logical material as well as with nonsense syllables. The greater value of distributed repetitions is particularly noticeable when material is tested two and four weeks after learning.

(2) For immediate recall, cumulative repetitions prove as effective as repetitions that are distributed.

(3) The forgetting of sense or logical material is rapid at first, then proceeds more slowly, as Ebbinghaus found for nonsense syllables.

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IN AID OF INTROSPECTION

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Introspection is neither an esoteric art which can be practised only by the initiated, nor an instinct placed by Nature in the breasts of all in order that the study of psychology might be possible. It is a scientific method. In spite of the inroads of Behaviorism, most of us still teach that introspecton is a very important psychological method. How many of us accept the implications of our position and *teach* that method? Text books on laboratory psychology give elaborate instructions designed to help the experimenter to the necessary technique, while it seems to be assumed that the much more difficult art of the observer will be "picked up." Now everyone can introspect, just as everyone can observe birds or flowers or any other natural phenomenon. But *scientific* observation implies a technique. This article is an effort to state the outlines of this technique in a form understandable to students. While I shall make no effort to conceal my own psychological articles of faith so far as these have been formulated, I shall attempt to state the method of introspection in a way which will be acceptable to those who do not have the same point of view in general psychology. At the same time, I shall offer a view of introspection which will not be acceptable to many. This view of introspection (as distinct from my general psychological position) cannot be separated from my account of the technique of introspection. One further caution: much that appears here will seem hackneyed or obvious to the professional psychologist. It has not seemed hackneyed to my students, nor am I aware of any account of the technique of introspection addressed to such readers.[1]

Introspection is a natural type of behavior. We all introspect frequently, just as we observe in other ways. For introspection is simply observing our own experiences. When we say "I feel warm," we have introspected. This may

[1] My obligations to G. E. Mueller and E. B. Titchener will be obvious to all who have followed the controversy aroused by Külpe's pupils. I owe much also to an unpublished laboratory manual by the late J. W. Baird.

or may not be scientific introspection, that is, observation which has complied with certain definite canons which make for accuracy and completeness.

The first difficulty is one of terminology. The words of daily life are woefully inadequate to scientific description. The first task of psychology is to refine terminology; the student's first task is to improve his power of distinguishing, classifying, describing, mental facts.

No real progress is possible in this task unless the student actually observes repeatedly the things described. The more you observe, the better you describe. The better you describe, the better you can observe and the more you will observe. This requirement is not peculiar to psychology. Compare the description of a flower made by the naturalist and the novice. Not merely is the first description clearer and more accurate in words; it is incomparably more complete in the details noted. To the beginner in any science, it seems discouragingly as if it were all a matter of "Words, words, words!" It is regrettable that we must spend so much time in the mere refinement of definition, but it is inevitable, not only for clear thought but also for complete observation.

Our next difficulty is that in psychology we do not observe things, but events or processes or happenings, the varying reactions of a mind or self to its environment. The thing you observe will not stand still while you watch. Indeed your watching is apt to make it change more rapidly; though one may add parenthetically that this last statement is by no means so universally true as would be implied from the majority of textbook discussions. The great trouble is not so much that introspection inhibits strong emotion, as that strong emotions tend to inhibit the introspective attitude. But the changing object of psychology is no peculiar difficulty. In many or most sciences, the object observed suffers changes rapid or slow. It is not inherently impossible to observe changing objects; it is merely more difficult. Laboratory devices may cause the object to change more slowly; increasing practice enables one to observe more and more of the swiftly moving panorama. One reason why students are introduced first to sensations rather than to affections is that they are relatively more stable, less swiftly moving. Finally we may take, as it were, moving pictures of the happening; each view is motionless, yet when taken in connection with others it closely simulates the real eventfulness of the experience. We may then proceed to examine a single view or snap-shot, to name the constituent features, to study their pattern or ar-

rangement. We may study the progress of these features from view to view. We must beware, of course, of thinking of mind as essentially thus a sort of kaleidoscope. The kaleidoscope is merely the way in which our minds represent and describe what is essentially a changing process.

It is further a unitary and not-divisible process, but this is not to say it is an unanalyzable one. I quote an illustration from Titchener;[2] a half-trained student reports in an experiment a feeling of "perplexity." Now perplexity is clearly a complex experience. A group of processes is present, some of which we can experience in other contexts, disjoined from each other. True, I have a fair idea of what he has experienced. But only a *fair* idea. The description should be so full and complete that one can imaginatively or sympathetically reconstruct the experience. Obviously, to say that you have "a feeling of perplexity" leaves much to the acuteness of one's imagination. Our first rule, therefore, is: *As far as possible, describe the constituent features of the experience in terms that resist further analysis. Describe in terms of part-processes which cannot be thought of as being themselves made up of smaller or simpler part-processes, or of part-processes found in other contexts.*

Is our task ended when we have completely described the momentary state of our experience in elementary or unanalyzable terms? By no means. Even Titchener, with his insistence upon the necessity of description as complete as possible in elementary terms, laments the current neglect of the durative or temporal aspect of experience. We must study the currents and eddies in the "stream of consciousness." Philosophers are not agreed whether we can directly observe change or can observe only sequence. The result is the same for our immediate purposes of psychological description and explanation. What we want is not merely the complete analysis of the isolated moment, but the movement of our experiences in time.

For psychology is not a purely descriptive science. We are not interested in description for its own sake. Psychology must help us to understand our complex selves, must supply the theoretic knowledge necessary to enable us to control our own behavior and that of others. To this end a description of the experience of a single moment, of a momentary snapshot of our experience, couched in elementary terms, is far

[2] E. B. Titchener, Description vs. Statement of Meaning, *Am. J. Psychol.*, xxiii, 1912, 167.

more useful than the average student is apt to realize. But it is not enough.

In the passage from which we quoted our illustration about perplexity, Titchener says that "the word 'puzzle' or 'perplexity' gives him the key to the observer's predicament, enables him to handle it, . . . but the word tells him nothing whatsoever of the observer's individual experience, of the particular 'feels' that constituted the perplexity in the particular case." Agreed! And we have just been arguing that the "particular feels" should be reported whenever possible. And yet if Titchener is right in saying that such an appellation as "perplexity" gives us "the key to the observer's predicament," do we greatly need the detailed analytic description? We do not want this description for its own sake, but precisely because it does give us "the import of the situation . . . enabling one to handle it." My criticism is that Titchener is too generous to the sort of report he is criticising. Such descriptive appellation is too crude a "key to the predicament" Analytic description would in most cases be much better. Yet just in so far as descriptive appellation does give us the "import of the situation" and enable us to handle it, it is very welcome.[3] I therefore add a second rule: *In addition to analytic description, experiences which are rapidly changing should be characterized or communicated by descriptive appellations, laying stress upon the sequences and order of the part-processes.*

I think it is because this kind of observation has been—not omitted, for it abounds in every piece of introspection, but rather—scorned and rejected of psychologists, that we have the weakness of experimental psychology on the dynamic side, on the side that considers time as a real factor in mental life. An example will show my meaning. In a certain investigation of mine, one of the subjects was thoroughly impregnated with the doctrine that introspection must be exclusively in terms of elementary experiences. Page after page of very excellent introspective analysis of his consciousness did he give me. But the whole thing was somehow dead; it did not *move*. In his desire to get all the processes analyzed into their elements, he was precluded by a sort of negative abstraction from attention to and report on the growth and move-

[3] It is possible that I have misinterpreted Titchener's meaning. The "predicament" of which he speaks may be wholly a laboratory predicament, the "key" merely a laboratory key. In any case I have to thank him for expressions so appropriate to the advantages of descriptive appellation.

ment of his experience. Only occasionally a statement about sequences would break through as it were surreptitiously. Analytic description is long and tedious. Be it never forgotten that it is invaluable; but a descriptive appellation can often give one a whole experience or some important part of it in a nutshell. And because it is so much briefer, it enables us so to observe the sequences that in the end we may get just that clue to the observer's predicament which is the common aim of both analytic and appellative description.

The danger of appellation is that we are tempted to infer analytic description from it. This we cannot safely do. These are parallel methods for arriving at explanation. Only where an appellation has been agreed upon as representing a certain analysis already made, may one justifiably draw any conclusions about "the particular feels in the particular case."

A further danger of characterization or appellation is the likelihood that it will be confused with interpretation. In my opinion, this is an overrated danger, but it exists. It is clearly illegitimate in your report of an experience to add one jot or tittle more than was in that experience. If, then, in your report, you add your interpretation of your experience, you have strayed from the truth. An example will suffice to show the nature of the error. In reporting the experiences incident upon rearranging a number of letters to make a word, an observer reported that "I shied off from that mass of consonants at the beginning, which could not possibly make a word." At best this is ambiguous. Did *O* mean that he framed more or less definitely the idea that such a mass of consonants could not make a word and that attention then shifted voluntarily to the end of the group? Cross-questioning brought out the true state of affairs: there was an easy non-voluntary shift of attention, following a slightly unpleasant perception of the many consonants. The whole clause "which could not possibly make a word" was an explanation after the fact. However true as explanation, such an interpretation is profoundly misleading if presented as a part of the consciousness of the given moment. Rule 3 therefore runs: *Include interpretation sparingly and always label it carefully as such.*[4]

As long ago as the 17th Century, Descartes called attention to the errors which slip into introspection, itself highly re-

[4] The above account of interpretation differs materially from Titchener's. I confess I do not understand what Titchener and his pupils mean by the term, but they clearly include what I have illustrated above and more.

liable, because of a persistent tendency to shift unconsciously from attention to experiences into attention to external objects.[5] As Titchener says, the error is both insidious and persistent. Introspection being defined as observation of one's individual experiences, everyone admits the impropriety of attempting to estimate the stimulus, which is an external thing. If introspection were to attempt judgments about the stimulus, its inadequacy would justify all the strictures that have been heaped upon it. How pitifully this pseudo-introspection falls short of accuracy, for example, in estimating the distance apart of two points of the esthesiometer! But that is not its proper task. What has introspection to do with physical measurements? Its task is to report on consciousness. In strict accuracy one ought to say, not "one point" or "two points," but "one pressure" or "two." No harm is done if it be clearly understood that "one point" as a report shall mean that the observer feels one clearly defined pressure. To insist otherwise would be to split hairs. Yet be sure you observe your convention. To slip over gradually into reporting "two points" whenever you *judge* that there were two stimuli leads to confusion. For such a report is not an unambiguous statement about your consciousness. You may have been aware of one pressure too large (so you judge) to have proceeded from one point. Yet according to your agreement with the experimenter, "two points" was to mean two distinct pressures; and this was not the case. On the face of the matter, the distinction seems trifling, but strict truth is never trifling. As a matter of fact, the failure to observe the distinction in this very experiment led to some very significant errors, and careful discrimination to their explanation. Rule 4: *Avoiding the "stimulus error," make no attempt to estimate the stimulus; confine your report to your consciousness, to your experiences. Nothing else is introspection; it is merely physical observation under difficulties.*

Rule 5: *Ordinarily describe experiences in their temporal order. But sacrifice this if necessary to catch some fleeting and elusive experience.* The advantages of this rule from the mere standpoint of convenience are too obvious to need comment. Its observance will also assist towards completeness.

Rule 6: *The experience or part of an experience selected for observation should not be too long, only a few seconds at the most.*

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[5] In his Meditations. See especially Meditation II, Everyman Ed., *Discourse on Method, etc.*, 90.

This rule requires one or two comments. It is impossible for even the most practised observer to remember with sufficient accuracy and completeness the very complex happenings of a longer period than a few seconds. But many of the experiences which we desire to have described and explained take more time to their occurrence. If we stop the experience in mid course and commence to report on the part that has just taken place, we have no adequate guarantee that the experience would have gone on to the end that we suppose. If we allow the experience to terminate naturally, the observer is apt to forget the temporal course, to omit elements, and even to insert factors that were not present. One partial solution is to emphasize descriptive appellation, but as we have seen, this is often far from adequate. No single solution is possible. The writer believes that actual interruption of an experience should be resorted to with great caution. In all such cases, the experience should *previously* have been allowed to run its natural course. In most cases a satisfactory compromise may be reached by selecting certain portions of the experience for particular attention and report, while relatively neglecting the rest. If this be repeated often enough to allow of particular attention to each temporal part of the experience, an accurate picture of the whole can be obtained.

Rule 7: *Avoid "putative recollection."* This is a very common error among beginners and is frequently committed by those who should know better. It is the sort of alleged observation which begins with "Well, I guess I must have had" a visual image, or what not. Logical inference has its place in psychology. Less primary than introspection, it is possible that it is more important. But logical inference has no place in observation. It is fatal to observation. Of course guessing is much easier than observing, but science is not to be founded on such a basis. It is no disgrace to reply to the experimenter's question with "I don't know." But nothing so openly betrays your ignorance and inexperience as the attempt to conceal it by guessing.

So much for a few hints on the "How?" of introspection. But the "What?" has been treated as if it were entirely obvious. Of course this is far from being the case. It is likely that the student will be first set to observing experiences predominantly sensational. What are you to say about these experiences? First, you must be prepared to make a comparative statement about their attributes. Secondly, you must be prepared to state the patterns of combination into

which the elementary experiences enter. Now psychologists are not agreed as to just what attributes we can assign to the various kinds of sensations. As observers, therefore, we must keep an open mind. We must have in mind all the attributes which any competent person has alleged to belong to the sensation and see if we can find them. There are seven candidates which are commonly urged for the office of sensational attributes. These are quality, intensity, extensity, durance, local sign or order, temporal order, clearness. Of these the first two are obvious in every sensation. No one questions that every sensation has some particular quality. Your task in observing a relatively simple experience is to *identify* the several qualities which compose it. Intensity is another attribute which is obvious to our untutored observation.[6] Here our problem is more difficult than with quality. Intensity is an attribute which carries from just more than zero to a maximum, while quality is strictly speaking invariable.[7] Hence we must make some sort of comparative statement about the intensity; we must measure it in terms of some scale of intensities. This was once deemed strictly impossible, though we all make such reports whenever we compare the loudness of two sounds. The possibility of accuracy in such determinations is another matter and one which depends upon the development of experimental methods, among others upon the avoidance by the *O* of the stimulus error referred to above.

As implied above, quality and intensity are very simple attributes and are open to very unpractised introspection. This is not always the case with the others. Thus local sign, while easily identified in the field of touch, and without great difficulty in vision, as the not-further-analyzable factor which differentiates one touch from another, or one tiny speck from another precisely like it imaged on another part of the retina, —while easily identified in these cases, is with great difficulty to be found in others, if at all. For if it is found in other senses, it is in Watt's phrase "wrapt up in the complications and modifications which experiences produce upon one another." To discover an attribute thus wrapt up, familiarity with that attribute in other senses where it is more obvious is implied, and may even then be very difficult. Not infre-

[6] Compare the illuminating discussion by H. J. Watt in the Introduction to *The Psychology of Sound*.

[7] This is to say, when the quality changes, we have a different sensation. This makes quality in some sense fundamental to the other attributes, and so it is, as men have recognized in naming sensations from the qualities.

quently we are obliged to state the derived, complex attribute which we find in the experience (such as its localization, which is derived, it is now commonly believed, from local sign and extensity in combination). Nevertheless we must in each case question our sensational experience for the presence of each of the seven attributes given above. Extensity or volume, the primitive "bigness" of a sensation, and durance or the primitive "lastingness" are, like intensity, attributes which vary from just more than zero to a maximum (whether within the range of one sensation or not appears to be an open question) and hence our problem is to measure them. In actual practice we are accustomed to measure the derived attributes based on these, extension and duration, and students sometimes find some little difficulty in limiting themselves to the simple attributes.[8] Local sign or order, and temporal order resemble qualities in that they do not vary from zero to a maximum. Yet these attributes, unlike quality, are quantitative, since they can be summed. Temporal order and durance are said by many psychologists to be derived, dependent upon the presence in consciousness of more than one sensation, and their uniform and necessary presence is denied. We may therefore question our experience as to whether these characteristics are present and if present as to whether they are simple.

Clearness is a characteristic of sensations whose systematic position is doubtful. These doubts need not concern us as observers. Every sensation has some degree of clearness and as introspectors it is our duty to determine how great this is.

There is yet another characteristic of many sensations, which is denied an attributive rank by most systematizers, but which should be reported on by the introspector in much the same way as the attributes. I refer to the hedonic tone or the amount of pleasantness or unpleasantness of the sensation. This point need not be labored, for it is the writer's experience that unpractised introspectors devote most of their energy to report on this factor.

We have then eight points on which to question our sensory experiences: quality, intensity, extensity, durance, local sign or order, temporal order, clearness, and feeling tone. At times, as was said above, the student will be unable to find these lying open to introspection, but will be obliged to report the presence of such derived attributes as show their presence indirectly. The two orders in particular are almost

[8] The student will probably find most help in establishing the distinction in W. James, *Principles of Psychology*, II, 1890, 135.

invariably found as spatial and temporal localization, derived attributes depending upon the co-operation of two or more experiences.

However satisfactory this may be for the systematizing psychologist in proving the presence of the primitive attribute, it is unsatisfactory from the standpoint of the second duty laid upon the observer of sensational experiences. Not only must we report upon the attributes of the sensations present in a given experience, but we must report the pattern or scheme of combination of these sensations. We want to know precisely what primitive attributes go to make up these more complex attributes. What besides extensity does enable us to localize? What aspects of an experience take a dominating place in consciousness? "A recent writer has declared that the 'texture' of qualitative perception, due to the 'massing' of its sensory elements,—it is difficult to find words to indicate precisely what is meant,—may, in certain spheres, be as important in creating apparent qualitative differences as is the quality of the single sensation."[9] Unfortunately little can be said to help the student in his effort to describe the pattern of these complexes beyond a renewed insistence upon the value of analysis, not as the end but as the beginning of this process.

It is time to give the student some encouragement. The number of things that seem to be required of him are many and the difficulties not slight. Were it not for one factor, the accomplishment of our task would be almost impossible. The intention to introspect, to attend to one's experience, materially facilitates its own task. Our powers of observing are heightened by direction of attention, especially with increasing practice and familiarity with the phenomena of the general kind to be observed. But in addition to heightening our power of observation, we get a sort of placing or classifying of the new experience in terms of familiar psychological rubrics or concepts. Sometimes the experience will be explicitly placed by means of a sentence in internal speech, sometimes there will be only a fleeting verbal gloss by means of which the new experience is assimilated to old and familiar classes. Often enough the chief instrument in such apperceptive classification (as it is called) is the name of the group to which the experience belongs. Thus Stricker writes: "When, after my experimental work, I proceed to its description, as a rule I reproduce in the first instance only words

[9] E. B. Titchener, *A Text Book of Psychology*, 349, quoting E. Murray, Organic Sensation, in *Am. J. Psychol.*, xx, 1909, 446.

which I had already associated with the perception of the various details of the observation whilst the latter was going on. For speech plays in all my observing so important a part that I ordinarily clothe phenomena in words as fast as I observe them."[10]

Apperception may take place in non-verbal ways, though perhaps introspective apperception is more effective if in words. Sometimes the experience is apperceived while it is running its course; more often, with the unpractised subject, it takes place immediately afterward.

Closely allied to this is a phenomenon which is probably most familiar in the field of hearing. You say something "back-end-to" and people laugh. Suddenly you realize with startling vividness what you have just said. It is not a peripheral thing like the after-images of vision and touch, which are more properly called after-sensations, yet it is more like these than ordinary memory or fancy images. Now under the influence of the intent to observe, a great proportion of our experience can be preserved in a memory after-image of this sort. Under experimental conditions it is highly reliable and greatly facilitates that completeness of report which would otherwise be so difficult.

It has been assumed in the foregoing that the reader is familiar with the general conditions necessary to any observation; care, accuracy, honesty, etc. The discerning cannot have failed to notice, however, the constant stress laid upon practice and the intention to observe the psychological facts. Indeed this intentional direction of attention, and the implied familiarity with the general nature of the phenomena to be observed, are the secret of valuable introspection. I began by saying that anyone can introspect. I conclude by urging that the value of one's introspection is almost directly proportionate to one's habitude. Anyone with a good memory and a sincere desire to improve can learn to introspect in a way which will be of distinct scientific usefulness.

[10] Quoted by Wm. James in *Psychology, Briefer Course*, 1892, 309.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

XLIX. An Experimental Study of Cutaneous Imagery

By Catherine Braddock

Very little experimental work has been done upon cutaneous imagery, and that little has no more than a preliminary value. The present study was undertaken to find out (*a*) if cutaneous images of pressure, warmth and cold occur, and in that event (*b*) if the images are free or tied, that is, if they are capable of sustaining by themselves alone the whole meaning of a past perception, or if they appear only in the train of other images (visual, auditory, kinaesthetic) which have proved to be thus capable. The outcome is so surprisingly negative that it, too must be regarded as only provisional. We have some good observers, but only a rough method.

Experiments: Series I

Our object in a first series of experiments was to obtain a direct comparison of sensory with its corresponding imaginal experience. The sensory stimuli were areal (2 by 1 cm.); metal blocks, rounded at the corners, for warmth and cold; wood covered by felt for pressure. We used two markedly different degrees (weak and strong) of the three qualities. For weak cold the metal block was cooled in water of 10° C, for strong cold in ice-water; for weak warmth the stimulus was heated in water of 4° C, and for strong warmth in water of 10° C, above the temperature of the skin. The cutaneous surfaces investigated were those of forearm, upper arm, forehead and cheek. *O* sat with closed eyes, and the stimulus was set down for 1 sec. Chance determined the order in which the six stimuli should be given. The instruction, in the sensory series, ran: "I shall stimulate the skin [of your forearm, etc.]. When the stimulus is removed, describe the cutaneous experience as well as you can." We thought that the reports of this sensory series might help in *O*'s training, as showing him what to look for in the image; we thought also that, if there should be differences between image and sensation, the differences would stand out the more clearly for the immediately previous experience of sensation. We realised that our heavy pressure stimulated deeper-lying organs, and that our temperature stimuli aroused pressure as well as cold and warmth. It seemed best, in a first attempt, to take these complications into the bargain.

At the conclusion of the sensory series on a given surface there was a pause of 5 min., and an imaginal series began. The instruction ran: "I want you now to think of one of the sensory experiences of the last series. When I have named it, realise it as fully as you can, and then describe it. Think of [strong cold, etc.]." Chance again determined the order.

In general it was found unnecessary to present a sensory series for the various surfaces more than once. Three imaginal series were performed for the four surfaces, so that there were 12 series in all. As these continued two pressures, two warm pressures, and two cold pressures there were possible 72 pressure-images, 24 cold-images and 24 warmth-images.

Series II

In a second series we asked *O* to realise familiar situations in which an image of pressure, cold or warmth might be expected to appear. It seemed possible that, under these conditions, images might be aroused with greater ease. The instruction ran: "Realise the following situation imaginally, and describe it as well as you can, paying particular attention to [cutaneous pressure, etc.]. Think of yourself as"—and then a situation would be given. There were ten situations for the three kinds of experience. We used for cold: stepping into a cold bath, having a snowflake fall on the face, holding a snowball in the hand, feeling a key slip down the back, stepping out of a warm house on a frosty day, laying a finger on a cold radiator, taking too large a spoonful of ice cream, waking up at night to find the bedclothes slipped down, having an icepack on the forehead, feeling a dog's nose against the palm. For warmth: bending over an open fire, stepping into a hot bath, putting the hand in the oven, taking a pot off the range, sipping warm cocoa, stirring a pan of boiling sugar, laying the finger on a warm radiator, clasping the hand round an electric bulb, touching a hot-water bottle with the foot, having a pad of cotton wool bound over the back of the hand. For pressure we tried to find situations which should, so far as possible, rule out kinaesthesis: the feeling of a leaf falling on the back of the hand, the feel of fur against the skin, the feel of an insect on the neck, lying on a stone, the touch of a hand on the shoulder, the feel of the wind beating on the face on a gusty day, a hair-shampoo, being brushed down by a porter, receiving a handful of change from a conductor, the flutter of a moth on the arm.

Observers

There were five *O*'s: Dr. L. B. Hoisington (H), instructor in psychology; Dr. H. G. Bishop (B), assistant in psychology; Mrs. A. K. Whitchurch (W) and Miss C. Braddock (Br), graduate students; and X, an untrained *O*. H and B had recently had extended training in the observation of kinaesthetic imagery.

General Results

(1) We cannot say positively, in the light of our results, whether images of cutaneous pressure, warmth and cold occur. If they exist, their appearance seems at best to be rare; and they are so interwoven with sensory experiences (changes in temperature and tension of the skin, kinaesthesis) and with visual and other images that they are very difficult to detect. The *O*'s differed greatly in their reports. W and B, in the majority of cases, were unable to obtain anything but very doubtful images; X, on the other hand, reported their occurrence readily. H and Br were also doubtful in a large proportion of their reports, but obtained at times what seemed like clear cases of

imagery. There may very well be individual differences. But there is no doubt that, in the experience of realising imaginally the former perception of stimulus or situation, meaning may be mistaken for process. Time and again H (our most practised *O*) would report that a former experience of pressure, warmth or cold had been adequately realised, and yet that he could not say whether an imaginal process in kind, a quality of pressure, warmth or cold, was present in the imaginal complex. H also frequently reported a tendency to say that he had obtained an image; he might even set himself to describe it; and then he would realise that some other image had carried the meaning of the cutaneous experience, and had thus suggested that the required qualitative image must be there. Br reported the same sort of thing.

W's and B's doubtful cases were largely of a different order. Many of them were reported as probably sensory and not imaginal, the sort of pressures and temperatures sensed when one attends keenly to a given part of the skin. Both *O*'s, however, and W in particular, reported cases like those of H and Br.[1]

(2) If cutaneous images occur, there is little doubt that they are tied and not free. Practically all the cases in which images appeared as free are cases in which the *O* reported a doubt whether his cutaneous experience was sensory or imaginal. Those cases, on the other hand, which approached nearest to true imagery (though we cannot vouch certainly even for them) were invariably tied either to visual or to kinaesthetic imagery.

Results in Detail

Pressure.—The reports of the *O*'s differ very greatly. H was more sure of his pressure-images than of the other two kinds. He and X found the images easy to get, and rarely failed to report them. W and B, on the other hand, were in most instances doubtful. Br in Series I regularly reported pressure-images, but became more and more doubtful of them in Series II; deep pressures (kinaesthetic) seemed to be involved. W and B often reported kinaesthesis, which appeared to serve as a substitute for cutaneous pressure.

All of H's reports in Series I contained pressure-images, many of them more than one. The reports began typically with a pressure-image, limited in extent and of brief duration, followed by visual imagery ("bit of lighter grey against a darker grey background") and eye-kinaesthesis, which together served to localise the pressure at the required spot upon the skin. "An experience realised in pressure-terms alone," H remarked at the end of the series, "is a generalised kind of thing, without concrete setting, whereas when the visual image is present it makes the experience definite and particular." The

[1] It is plain that results like those of G. H. Betts (The Distribution and Functions of Mental Imagery, 1909, 45 f.) must be taken with many grains of salt. Nor is it of any use to improve one's questionary; the fault lies, not with the questionary, but with those who are called upon to answer its questions. X, our untrained *O*, gives consistently positive results (in full agreement with those of Betts), and yet it is certain that, whether or not imaginal processes were present, he was reporting meanings and not processes. We must in fairness add that much water has flowed under the bridges since 1909, and that Betts would hardly write to-day as confidently as he wrote a dozen years ago.

pressure-image usually recurred after the localising and particularising had taken place. Sometimes visual imagery or eye-kinaesthesis or auditory-motor processes might occur before the pressure-image appeared, and sometimes the visual and pressure images alternated several times before H was satisfied that his realisation was adequate.

Here, then, there seems to be a clear-cut case of tied pressure-imagery, with visual imagery and eye-kinaesthesis serving as support. We noticed, however, at the beginning of Series II, that H did not (apparently) distinguish between pressure and kinaesthetic imagery; pressure was often reported as "meaning movement."[2] Since the other *O*'s frequently reported kinaesthesis as such, we thought the point worthy of further experiment. We therefore performed short sensory and imaginal series on subcutaneous pressures (etherised skin) and the sensations involved in raising the arm. In the imaginal series the deep pressures became 'surfacy,' bidimensional, though they were not necessarily localised at the surface of the skin. The results appear to show that, at any rate under these rough conditions, there is for H no difference, in process-terms, between subcutaneous and cutaneous pressure.

If this interpretation is correct, H's reports square very well with those of Br. Here the typical report is that of a visual image representing the stimulus, followed by a deep pressure, referred by the *O* to kinaesthesis. In the effort after full realisation of the perceptive experience these images alternated, the pressure coming nearer and nearer to the surface, until finally it appeared as a flash-like process on the skin itself. The visual image of the stimulus was necessary to *O*'s realisation of the corresponding perception. The flash-like pressures were (as has been said) at first accepted as cutaneous; later on the *O* became very doubtful of their validity.

B, on the other hand, reported typically pressures of large area and long duration, which seemed to be sensory in nature, and to resemble the pressures brought out by sustained attention to the skin. It is possible, however, that kinaesthetic imagery was involved; since, after a supplementary series on deep-seated pressures (similar to that taken with H), B remarked that all the pressures of Series I and II had extended a little below the surface of the skin. Kinaesthesis itself was often reported. B was reluctant, throughout the experiment, to commit himself to an imaginal report.[3]

W relied for realisation of her perceptive experiences mainly on visual imagery. She also reported a great deal of kinaesthesis. Her usual 'cutaneous images' were of the same sensory nature as B's. She remarked in both series that if there were no visual image there would certainly be no cutaneous; and in Series II she reported no cutaneous image that was not possibly sensory.

We thus have three possibilities as regards the image of cutaneous pressure.

(1) True images exist, but occur only as bound up with visual or kinaesthetic images. Evidence for this view may be found in the reports of H and X (visual support), and in many of the reports

[2] In the situation of stepping into a cold bath, movement in the leg and shiver in the back were gross enough to be noticed by *E*, although H reported pressure only.

[3] We have tried to tabulate the reports, but have found the task impossible. The *O*'s 'hedge' and qualify from the start, and grow more and more doubtful as the experiment proceeds.

of Br (kinaesthetic support), and in a few reports of W (visual support).

(2) Deep-pressure images are mistaken for images of cutaneous pressure. This view accords best with Br's results, and in part with those of B. The meanings of skin-surface, localisation, and extension, are carried in visual terms.

(3) Sensory kinaesthesis is mistaken for imaginal cutaneous pressure, and together with visual imagery carries the meaning of skin-surface, localisation, extension. Evidence for this view may be found in the reports of X, W and B; to some extent also in those of Br.

It is possible to read H's results in the sense of (2) and (3), especially since we know that H was set to report qualities, and (as he told us later) avoided the term 'kinaesthetic' owing to its merely functional meaning. As Br and W became increasingly doubtful with practice, and as X's reports may be discounted, the evidence for (1) is very slender. We have no doubt, on the other hand, that (2) and (3) cover part of the facts; the only question is whether they are adequate to the whole.

Warmth and Cold.—All the *O*'s except X were uncertain of these images; every report has some measure of doubt attaching to it. H and W were less sure of cold than of warmth, Br was less sure of warmth than of cold. The reports fall into four more or less distinct groups: (*a*) a deep intense warmth or cold with something pricking or stingy about it; (*b*) a diffuse experience, like a breath of warm or cold air sweeping over a largeish area of the skin (fair intensity, long duration), which runs into sensory warmths or colds elsewhere in the body; (*c*) an experience like a puff of warm or cold air, small in extent, very weak in intensity, short in duration; and (*d*) an experience of pressure, with warmth or cold inherent in it. In the case of warmth there was a variant of (*b*), in which the warmth began in a small area and spread gradually in waves till it merged with the bodily warmth elsewhere.

The experience reported in (*a*) occurs but rarely (H, Br, W). It seems to be kinaesthetic in nature. Experience (*b*) is often reported by all *O*'s, and is typical for B. It seems to be obviously sensory; indeed, the qualification "perhaps sensory" is appended to many of the reports. The variant is also probably sensory, at least in some measure. It occurred typically in X's reports, always bound up with visual or pressure imagery.

There remain (*c*) and (*d*), in which if anywhere we shall find true images of warmth and cold. The experiences of (*c*) were extremely vague and elusive, "just a flash and then gone," so that the *O*'s found description almost impossible. Since the stimuli of Series I combined temperature with pressure, there was greater variety under (*d*): pressure might appear without warmth or cold, pressure might immediately precede or follow warmth or cold, pressure and warmth or cold might occur together, at roughly the same intensity and clearness, or pressure might be predominant, with a tinge of warmth or cold added to it. H and Br report all these modes of experience, though with the recurring doubt of the imaginal character of warmth and cold; W and B were too much in doubt to do more than report a possible temperature as given with the visual image or the pressure.

Visual images were important for all *O*'s except B, and were indispensable to W. In the latter part of Series I, H and Br tended to drop vision for pressure (kinaesthesis?), but the visual images were

prominent again in Series II. If temperature images occurred, they were certainly tied, either to vision or to pressure. We did not experiment with radiant heat or cold.

Other Factors.—In both the sensory and the imaginal series H and W frequently reported the kinaesthesis of eye-movement, accompanied or followed by visual imagery. The report was less common with B and Br.

Auditory-motor (verbal) processes played an important part in H's reports. They often appeared at the beginning of his train of imagery, and carried a generalised reference to the previous experience. In the temperature series they might further carry the particular meaning of cold or warmth, and thus render it doubtful whether an imaginal temperature was experienced. They also carried the meaning of confirmation, acceptance or denial of the adequacy of a given process-complex as realisation of the former experience.

B relied largely upon auditory-motor processes; they might so fully mean the past experience that no other processes of any kind were reportable. In Series II B introduced auditory-motor images into the situations, in order by their aid to make his task easier. He reports more adequate realisations in this Series than in Series I, though his cutaneous imagery does not become more assured.

The other *O*'s have little to say of auditory-motor processes.

The Methods.—The method of Series I is a variant of that recommended by J. R. Angell.[4] Preliminary work with blunt points had led to very confused and unsatisfactory results; we therefore enlarged the area of stimulation, in order to give form and substance to the perception and a better foothold to the image. The method of Series II was suggested by the work of A. H. Sullivan on kinaesthetic imagery.[5] We have no doubt, in view of the competence of our four principal *O*'s, that either method would have detected the presence of free cutaneous imagery. None of these *O*'s, in fact, complained of the methods; recall and realisation were feasible enough; the difficulty lay in making sure of the image. Advance thus seems to depend rather on a sharpening of *O*'s *Einstellung* than on improvement of technique.

Conclusions

A conservative reading of our results leads to the following conclusion. Where *O*'s imaginal type tends to be one-sided (B, W), the imagery ordinarily employed comes up so readily and so dominantly that any possible traces of cutaneous imagery are obliterated; the *O*'s report substitutive imagery only. Where the type is generalised (Br, H), cutaneous imagery of pressure, warmth and cold occurs rarely in subordination to free imagery of vision or kinaesthesis.

This is the conclusion that Br and H themselves incline to accept. Both *O*'s doubt the occurrence of cutaneous imagery in the situations of everyday life; both are disposed to think that under the conditions of the experiment they sometimes obtained it.

There is, however, no proof of this position; and there is full proof of the trap set by meaning for the untrained or momentarily unwary observer.

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[4] Report of the Committee of the American Psychological Association on the Standardising of Procedure in Experimental Tests, 1910, 94 f.

[5] This Journal, xxxii., 1921, 54 ff.

L. The Integration of Punctiform Cold and Pressure

By S. Tung

The experiments described in this paper were undertaken to discover what psychologically results from the simultaneous stimulation of cold and pressure spots. So far as we are aware no previous attempt has been made to synthetize cold and pressure with punctiform stimulation. Some investigators, however, have said that the areal stimulation of the organs of these two qualities gives the experience of wetness. Helmholtz, for instance, remarks that "das scheinbar einfache Gefühl des Nassen, welches ein berührter Körper erzeugt, . . . aus dem des Glatten und des Kalten zusammengesetzt [ist],"[1] and Bentley has shown that under experimental conditions wetness may in fact result from stimulation with a dry smooth object (such as rubber sheeting) the temperature of which is either above or below that of the skin.[2] Thunberg, on the other hand, writes: "Eine Kälteempfindung, welche von einer Berührungsempfindung nicht begleitet ist, wird häufig als von einer Flüssigkeit verursacht aufgefasst, was daraus erklärt werden kann, dass beim Niedertauchen eines Körperteiles in eine Flüssigkeit der Drucksinn fast gar nicht erregt wird, dass also die entstehenden Temperaturempfindungen ungemischt sind. Im Zusammenhang damit steht eine eigentümliche Illusion von Nässe, welche entsteht wenn man die Stirn mit einem sehr kalten Gegenstande z. B. 20 Sek. berührt. Nach dessen Fortnahme dauert die Kälteempfindung noch einige Zeit fort und zugleich hat man den Eindruck, dass die Stirnhaut deutlich nass sei."[3] Fröbes says, even more emphatically: "Die Qualität 'nass' scheint nichts anders als einfache Kälteempfindung ohne gleichzeitige Berührungsempfindung."[4] The difference of opinion expressed by these authors can, of course, be settled only by experiment, and since we employed a punctiform and not an areal mode of stimulation, we are unable definitely to settle it. We may say, however, that under our conditions wetness (or, more accurately, a 'cold wetness') was found frequently to occur with simultaneous stimulation of pressure and cold spots, and that the experience was never observed when spots adequate only to their normal stimulus were excited singly. We now give a more detailed account of our methods and a more complete statement of our results.

Our first method was to stimulate with a cold-point spots on the surface of the fore-arm which responded both to pressure and to

[1] H. Helmholtz, *Physiologische Optik*, 1866, 698; 1896, 845.

[2] M. Bentley, The Synthetic Experiment, this Journal, xi, 1900, 414ff. See also E. B. Titchener, *Beginner's Psychology*, 1915, 47.

[3] T. Thunberg, in Nagel's *Handbuch der Physiologie des Menschen*, iii, 1905, 708.

[4] J. Fröbes, *Lehrbuch der experimentellen Psychologie*, i, 1917, 134. No authority is cited for this statement, but we suspect that it was taken from Thunberg's article. Thunberg does not mention Bentley's paper.

cold. We had planned to check the results thus obtained by stimulating in the same way cold spots that did not respond also to pressure, but we were unable to find spots of this kind because the intensity of pressure furnished by our apparatus was too great. As stimulus we employed a thermaesthesiometer with cold water running through the point. This was attached to a spring balance modeled after Thunberg's algesometer,[5] and the whole was mounted on a universal standard. The amount of pressure, as measured on a pressure balance, was 16 gr., with an mv. of 10% due to the difficulty of controlling adequately the weight of the water conduit. The temperature of the water varied between 18° and 21° C., and that of the room in which we worked between 21° and 26° C.[6] The point of the aesthesiometer was smoothly rounded and measured 1 mm. in diam. The fore-arm was placed in a warm plaster cast. The spots to be stimulated were permanently marked by tattooing. The observers were Professor Weld (W), Dr. L. B. Hoisington (H), Mr. S. Takaki (Ta), and the writer (Tu); Mr. Takaki served as *E* for the latter. The instruction given to the *O*'s was as follows: "I shall stimulate a spot on your arm, and you are asked to report the course, particularly with regard to quality and intensity, of the cutaneous experience."

Results obtained by the First Method. In early experiments the course of the experience was, in general, described by all *O*'s as, first, a pressure of low intensity which quickly passed into a cold: this, after an interval, changed into another cold which then, gradually and with fluctuations, faded out. Later the two qualities of cold were distinguished, the first as a superficial 'wet' cold, the second as 'just cold,' deep and penetrating. Typical characterizations of these colds are as follows:

W. "I felt chill, like a piece of ice touching the skin, which first penetrated deeply then quickly spread out." "Wet was felt but it disappeared rather quickly, then a deep cold that was not in the least wet." "I had first the impression of wetness like a drop of cold water."

H. "At intermediate intensity I felt something like a piece of ice with a strip of cloth surrounding it. Wetness was not sharply felt except at the edge of the spot; it did not last long. After wetness was gone, just cold was left behind." "The wetness was like water; as I felt it less as the cold became more intense." "The wetness was on the surface; the cold beneath the surface of the skin."

Ta. "I felt something smooth wet and cold at the spot like ice melting; . . . after wetness was gone I felt simply cold like the coldness of metal."

The wet cold was reported in about 80% of the trials; in some instances cold did not appear at all; in others it was long delayed, as a result, we suppose, of a relative displacement of the cold-organ and the tattooed spot. It was a logical inference that the change from the wet to the dry cold might be due to the earlier adaptation of the pressure, and we found by rough experiments that the average adaptation time of pressure was in fact only about ¼ that of cold. If the above inference is correct, we should be able to obtain a return of the wet cold by restimulation of the spot with a pressure-stimulus

[5] T. Thunberg, *Skand. Archiv.*, xii, 1902, 424.

[6] A lower temperature of the water was avoided because of the production of moisture by condensation on the metal point.

after the wet cold has gone but before the second cold has disappeared. We tried this experiment, and in some instances were successful. It was essential to success to employ a relatively weak stimulus (a hair), to apply it as close to the cold stimulus as possible, and to direct *O*'s attention to the cold; if the cold was not focal, a pressure was reported within an area of cold.

Our second method was that of simultaneous stimulation of discrete pressure and cold spots. For pressure we employed hairs of 3.0 and 4.2 gr/mm tension value; for cold it was essential to our method to excite the cold spot without touching the skin. In order to do this we found by trial that exceptionally low temperatures of stimulus were necessary; and the only one that proved adequate was obtained from a mixture of ether and carbon-dioxide snow.[7] This was managed in the following way: a Willyoung thermaesthesiometer, with thermometer removed, was clamped to a universal standard; it was first filled with the snow, and then the ether was added. When this was done the temperature of the mixture fell quickly to about —78° C., a heavy vapor streamed down perpendicularly from the instrument, and a thick frost accumulated on it. Since the vapor was felt as a 'damp cold,' and falling particles of frost as 'wet cold,' we were forced to wait until the violent stage of evaporation was over; thereupon the stimulus was ready for use. Our subsequent procedure was as follows: the cold stimulus was first lowered and adjusted in such fashion that the point of the instrument was directly over a cold spot. The distance above the surface of the skin varied from 2 mm. to 10 mm., depending upon the state of the mixture or upon the readiness of response of the cold spot. When *O* had reported a cold of good intensity, a signal was given, and a pressure spot in the immediate neighborhood of the cold spot was stimulated by the hair. *O* was instructed to describe the qualitative course of the cold after the signal.

Results obtained by the Second Method. We again frequently, although not invariably, obtained reports of 'wet cold.' At times, however, the pressure was not realised either as pressure or as anything else, and at others pressure was felt in an area of cold. These results seemed to be due either to a lapse of attention to the cold (a condition which our instruction was designed to meet), or else to the lack of a proper proportion of the intensities of cold and pressure; for example, we found that the pressure was most frequently not felt when the intensity of cold was strong, and that it generally appeared in a spatial pattern with cold when the cold was weak. We regret that we were unable, for want of time, to make a systematic study of this point. But it appears probable that the appearance of 'wet cold' is, aside from the mode of stimulation, conditioned both upon the greater clearness of cold and upon a certain relation between the intensities of cold and pressure. When the 'wet cold' occurred it came sud-

[7] We tried without success the apparatus of the first method with the point cooled by an ice-salt solution, a temperature cylinder also cooled by an ice-salt solution, a mica shield with small holes in it for receiving the pressure stimulus and for conducting cold from a block of ice, and an ice-ammonium chloride solution in the Willyoung apparatus. The department of physics in Cornell University kindly supplied the carbon-dioxide snow, and gave us the temperature of the mixture.

denly, with surprising distinctness, and lasted for a very brief interval. It was characterised as like a "melting flake of snow," "a drop of cold water," "a tiny particle of ice." The 'radiant cold' on the other hand was reported as "not punctiform but areal, less intense, and more penetrating than the 'wet cold;'" "it was like the 'dry cold' of the other experiment," "like the feel of a cool breeze without pressure, or the cold of evaporating ether."

The fact that 'wet cold' may derive from a simultaneous stimulation of cold and pressure spots is, of course, only a single result. The duration of the experience was, under our conditions, so brief that our *O*'s were baffled in their attempts to give a qualitative description of wetness. But an investigation of the synthesis of pressure and warmth with punctiform stimulation is now being made in the laboratory, and a systematic study of Thunberg's illusion is also under way. These two inquiries should throw further light upon that problem.

LI. The Hering Color-Blindness Apparatus and the Normal Rayleigh Equation

By M. Winfield and C. Strong

In a previous article on the Hering Color-Blindness Apparatus reference was made to experiments then in progress on the normal Rayleigh equation, the matching of spectral red and green to yellow.[1] In this paper we report the results of those experiments. It was our purpose not only to test further the capabilities of the Hering instrument, but also to determine the range and form of distribution of the matches of a large number of *O*'s who have normal color vision. That there is a wide variation among normal *O*'s has, of course, been known since Rayleigh first performed his experiment,[2] but interest has centered upon the extremes of variation (the anomalous trichromats), and the central variations have for the most part been either ignored or else explained away as the result of 'errors of observation.'

Conditions and Procedure of the Experiment. The experiment was performed in a dark room, and the apparatus was illuminated by an artificial light of Gage's glass[3] as described by Cowdrick and Winfield.[4] The horizontal distance from the center of the roundel to the light chamber was 13 cm.; the vertical distance from the top of the table upon which the apparatus stood to the center of the roundel was 27 cm. It was found by test that a variation of from 2 to 12 volts of current below the 110 at our disposal did not affect the results of the experiment. *O*'s eyes were protected from the glare of the lamp by a screen.

In making the equation we place a green glass in the left (to one facing the open side of the instrument), a red glass in the floor, and a yellow glass in the right aperture of the light chamber. The red reflector is set at 20° and the yellow at 15° of arc;[5] the former is

[1] M. Cowdrick and M. Winfield, The Adjustment of the Hering Color-blindness Apparatus, this Journal, xxx, 1919, 418.

[2] Rayleigh, Experiments on Colour, *Nature*, xxv, 1882, 64; On Defective Colour Vision, *Brit. Assoc. Rep.*, 1890, 728. For a review of subsequent experimentation see J. von Kries, in Nagel's *Handbuch d. Physiologie des Menschen*, 1905, 124ff., and in Helmholtz' *Physiologische Optik*, ii, 1911, 343ff.; J. H. Parsons, *Colour Vision*, 1915, 182ff.

[3] We have recently learned that not all roundels of Gage's glass have, as have those employed in this and former experiments in the Cornell laboratory, a ground surface. At the present time the manufacturers supply the glass as it comes from the press unless a ground surface is specified.

[4] *Op. cit.* They give incorrectly the horizontal distance from roundel to light chamber as 'about 30' cm.

[5] This setting of the red reflector was chosen because it permitted a green of excellent chroma at the one extreme of the variable. The 'mixing value' of R in this apparatus is much greater than that of G; *e.g.*, if the red reflector had been set at 40° of arc all the green

not moved throughout the experiment, and the latter is moved only when, after the match in hue has been made, *O* reports a difference in tint. The green reflector which conditions the variable stimulus is set to give R or G as *E* desires. *O* is then instructed to make the equation by turning the knob at his right hand (which controls the green reflector) to the right or to the left, as the case may be, until the match is made. In these experiments ten successive matches were required of every *O*, and the setting of the green reflector for every match was made according to the schema Rl, Gs, Rm, Gl, Gs, Rm, Gm, Rl, Gl, Rs, the capital letters signifying a setting of the reflector that would give R or G, and the small letters long short and medium distances from the approximate degree of arc that would give the match. In order to avoid the effect of local adaptation (which is an important source of variation in a single set of matches) *O* after making a match was required to rest his eyes for a few moments and then to verify, and if necessary to rectify, the equation before the scale-reading was recorded.[6] In all matches *O* used but one, usually the right, eye.

Results. Our results fall, according to the aim of our investigation, into two groups: those which concern the capabilities of the Hering Color-blindness Apparatus, and those which indicate the range and distribution of the matches of normal *O*'s. As regards the first of these, we found early in our experimentation that the apparatus, when employed under the conditions we have described, gives homogeneous results; but that, owing to the mechanical adjustment which controls the movements of the reflector, the units of measurement are too large. The adjustment is so 'coarse' that a change in the position of the reflector by less than a degree of arc, an amount that frequently destroys a match, is not easily made. The effect of this is, of course, to reduce the range of positions that for a single *O* will give a match; for example, 1% of our *O*'s gave the same reading 10 times, 2% 9 times, 19% 6 times, and 32% 5 times of a total of ten trials. This quantitative result is, however, conditioned in part upon the fact that *E* read the scale to the nearest degree, a procedure that, in view of other faults in the apparatus, seemed advisable. The size of the arc that carries the scale is so small that *E* could not read to less than ½°, and the distance of the pointer above the scale is so great that unless *E*'s eye is directly above the pointer the error of reading the scale is at least 1°. In the course of our experiments we reduced the error of observation to less than ½° by a five-fold extension and enlargement of both scale and pointer; but for the sake of uniformity we continued, as we had begun, to read to the nearest degree. In all other respects the instrument was entirely satisfactory for our purpose; it is easily managed, instantly ready for use, and if its position relative to the source of illumination is permanently fixed is always reliable.

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at our disposal would not have cancelled the red. Our experience has shown that a setting of the red reflector at 25° of arc would have given an R that could have been cancelled by all of our *O*'s, and the resultant yellow would have been of lighter tint and better chroma than that which we obtained in this experiment.

[6] In an extended series of observations with a single *O* who adapts slowly in the dark we found that general adaptation upon coming from a light into the dark room did not, after the first three or four minutes, make any sensible variation in the average equations.

As regards the distribution of the matches of normal *O*'s, we give the results of 100 cases. Many more than these were collected; but we have excluded not only those of the preliminary period before we had the apparatus and method of observation under control, but also 10 cases we found to be abnormal in the sense that they were either color-blind or color-weak. When once the final method, which we have described above, was determined upon, we began the collection of the 100 cases, accepting every *O* as he came, and continuing until the hundreth match was obtained. The *O*'s consisted of 36 men and 64 women taken as we found them from the graduate and undergraduate students and from the staff of the department of psychology in Cornell University. We believe that they as a group constitute a fair sample of a university population, and that every individual in the group has normal color-vision. The majority, however, were not tested specifically for color-blindness or color-weakness because indications of abnormality appear in the ordinary course of matching R and G to Y; abnormal *O*'s almost invariably give larger *mv*'s than normal, and they also give an average match that falls at the limits of (or outside) the normal distribution. We, therefore, tested for color-blindness or color-weakness only those individuals whose matches differed markedly from the average, whose *mv*'s were exceptional, and whose color-names in making the match were incorrect; the remainder we assumed to be normal. We give in the following table the distribution of the matches of these normal *O*'s.

TABLE I

DISTRIBUTION OF THE AVERAGE AMOUNTS OF G REQUIRED BY 100 OBSERVERS TO EQUATE R TO Y

Degrees of Arc.....	45	46	47	48	49	50	51	52
No. of cases.....	1	7	18	23	27	17	6	1

Average = 48.48 Median = 49

The difference between groups represented in the table by apposed degrees of arc is, we believe, highly significant. In the first place, the units of measurement were, as we have seen, too large to admit of minute gradations. Furthermore, the *mv*'s from the averages of every ten matches were small; the average of all these was 0.7°, and their median value was 0.6°; 27% of the *O*'s had an *mv* of 0.5° or less, and only 15% of 1° or more. Finally, if only the conditions were kept constant, we found no greater variation in average matches of a single *O* from day to day. We believe also that, in so far as our experiments go, we have determined the limits of normality; we found no case, normal or abnormal, beyond the limit of 45° (*i.e.*, in the direction of less green); and although nine cases fell beyond 52°, they were all abnormal; only one abnormal, a protanope whose average match was 52°, came within the limits of the normal distribution. If, then, the difference between groups is a real difference, and if we have been successful in finding the limits of normality, the range of distribution becomes significant. The mode is at 49° and the limits are 45° and 52°; there is, therefore a greater

range of distribution on the side of less green than on that of more green. In this respect the form of the curve is like one of 59 cases published by Donders, and consequently it may be that this form is typical.[7] But our own curve permits of another interpretation. It will be observed that, with the exception of the extreme groups, the difference in size of groups becomes progressively less as the mode is approached. Furthermore, the average of all matches is 48.48°, and this average falls between the two largest groups which combined equal half the cases. We might regard the curve, therefore, as broad and flat at the mode, and as falling away equally on the two sides. If, as von Kries suggests,[8] this variation among normal *O*'s has a physical cause, *i.e.*, is due to differences in macular pigmentation, there would seem to be no reason why the curve should not approximate to a normal distribution.

We do not wish, however, to overemphasize the form of distribution. The essential feature of our results is that the extent of variation among normal *O*'s has been brought into bold relief. The fact has, of course, long been known to everyone who has observed students, in demonstration of the first law of color mixture, match R and G to grey; but so far as our reading has gone von Kries is alone in realizing that the variation is distinctive enough to mark off the normal from the anomalous groups.

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[7] F. C. Donders, Farbengleichungen, *Arch. f. Physiol.*, 1884, 521.

[8] J. von Kries, in Helmholtz' *Handbuch der physiologische Optik*, ii, 1911, 345.

LII. The After-Effect of Seen Movement When the Whole Visual Field is Filled by a Moving Stimulus

By Wellington A. Thalman

In his study of the after-effect of seen movement A. Wohlgemuth reports a series of 34 experiments whose results he uses to disprove current theories and to support his own theoretical construction.[1] One of these experiments, the 22d, is devised "to ascertain how the after-effect is affected if the whole visual field is filled by an objective movement." The outcome is negative: there is "a total absence of any after-effect." Wohlgemuth, apparently dissatisfied with this finding, repeats the experiment, with identical result. He accordingly concludes that "no after-effect is produced by an objective movement occupying the whole visual field."[2] Later, in his theoretical sections, he reviews and rejects all current theories "either for *a priori* reasons or on account of incompatibility with experimental evidence," and proceeds, on the basis of McDougall's theory of drainage, to the construction of a theory which shall be "in harmony with the ascertained facts."[3] But in relating this theory to the facts *he fails to consider the results of his own Experiment 22.*[4] Yet these results, if they stand, appear to contradict the theory, according to which the phenomenon is in no way conditioned upon size or spread of the stimulus but is wholly dependent on priority of entry and the consequent decrease of synaptic resistance. If the after-effect fails to appear when the whole field is stimulated, the theory must either be discarded or radically revised.

We hold no brief for the theory; but we were not either convinced by the outcome of the critical experiment, which we therefore decided to repeat. We hoped also, by extending the method, by requiring introspective reports of process as well as statements of meaning, and by improving the apparatus and technique, to contribute something toward a complete phenomenology of the illusion.

Observers.—We had two groups of *O*'s. The members of the first group were Mr. W. A. Andrews (A), and Mr. S. Takaki (T), graduate students in psychology; Mrs. I. G. Whitchurch (W), graduate scholar in psychology; Dr. F. L. Dimmick (Di), assistant in psychology; and Dr. K. M. Dallenbach (Da), instructor in psychology. We gave these *O*'s a preliminary practice-series that should bring them

[1] *Brit. Journ. Psych.*, Mon. Suppl. 1, 1911, 1-117.

[2] *Op. cit.*, 72, 113.

[3] *Op. cit.*, 98 f. For McDougall's drainage-theory see *Brain*, xxvi, 1903, 153 ff.

[4] The experiment is referred to by number on p. 106; but the "Experiments 20-22" there mentioned should undoubtedly be "20, 21," since we read immediately after of "these two [not three] cases," and since the separate discussion on p. 109 mentions only Exps. 20 and 21. The slip on p. 106 is unfortunate, as it led the author to leave Exp. 22 out of account.

to the pitch of proficiency that Wohlgemuth's *O*'s had attained.[5] They were required to report on both process and meaning. The members of the second group, whom we shall quote only by number, were with two exceptions students in the summer session of 1920. These *O*'s approached the problem without special training, and were asked to report only upon meaning.

Preliminary Practice.—We used in the preliminary work a demonstrational model of James' artificial waterfall,[6] with and without a fixation point, in both the vertical and the horizontal positions. The curtain was rotated by hand at two speeds, fast and slow, and the rotation was continued for 5, 10, 15, 20 or 25 sec. There were thus 40 possible experiments; and as every experiment was repeated 5 times, every *O* gave 200 reports upon the illusion. Observation was binocular, at a distance of 25 cm. An adjustable biting-board served to steady *O*'s head and aided him to maintain his fixation.

The results of this practice-series may be summed up as follows. (1) All *O*'s obtained the illusion of backward movement. (2) The after-effect became more pronounced as practice advanced. (3) The after-effect became more pronounced as the exposure grew longer and the rate of rotation faster. (4) The presence or absence of a fixation point made no difference in the illusion. (5) The after-effect was as marked with horizontal as with vertical movement of the stimulus.

Repetition of Wohlgemuth's Observations.—We next set up Wohlgemuth's apparatus and repeated his observations. The apparatus,[7] which consisted of an endless band of printed calico of alternating black and white stripes 5 mm. wide, 180 cm. broad and 110 cm. high, stretched between two rollers, was placed in a dark room and was so illuminated by daylight mazda lamps that no shadows fell upon its surface. The band was driven by an electric motor which, by means of a multiple speed-reducer, moved it downward at a velocity of 12 cm. per sec.[8] The times of stimulation were 30, 45 and 60 sec.[9]

No one of our *O*'s found the entire field of vision filled by the black and white surface, even though he stood so close that his nose actually touched the cloth. Always there was an unfilled margin. It may be that Wohlgemuth's *O*'s failed to report this fact; it may be that Wohlgemuth regarded it as of no consequence. Since, in his second attack upon the problem, he reduced the dimensions of the stimulus-field to a square of 80 cm. side,[10] it is probable that he aimed

[5] On the importance of practice see Wohlgemuth, *op. cit.*, 27, 110.

[6] W. James, *Mind*, O. S. xii, 1887, 517; *Principles of Psych.*, ii, 1890, 245.

[7] *Op. cit.*, 72.

[8] Wohlgemuth does not state his rate of movement. We chose that of 12 cm. per sec. because his experiments on the "influence of the velocity of the stimulating movement upon the after-effect" (Exps. 10-13, pp. 45-52) showed that the optimal rate lay between 6 and 12 cm. per sec. (see Fig. 8, p. 50). These experiments were performed with central vision. Our own experiment involves peripheral vision, and we therefore thought it best to take the upper limit.

[9] We used these long times, although the preliminary practice-series had proved them unnecessary, in order to conform with Wohlgemuth's procedure; see *op. cit.*, 72.

[10] *Ibid.*

only at a relative filling of the field of vision. Wohlgemuth, again, used a fixation point throughout his observations.[11] It is clear that this point, with the wire that held it in place, further violates the conditions of complete objective movement. Our practice experiments had shown that the fixation point is unnecessary, and we therefore discarded it in part of our own work.

We performed three series of experiments with this apparatus. In the first we sought exactly to duplicate his conditions, and therefore introduced a fixation point. A piece of wire carrying a small white square of 5 mm. side was let down from above and ended at the center of the field, about 1 cm. in front of the cloth. In the second and third series the fixation point was removed. In the second, the *O*'s were directed to fixate the surface of the moving field; in the third, they were directed to look through and beyond the cloth, to fixate at 'infinity.'

The following instructions were read to the *O*'s at the beginning of every observation-hour: "Take a position about 10 cm. in front of the center of the screen. When you have done this, close your eyes and wait until the experimenter says 'Ready,' 'Now.' On the 'Now' open your eyes and (*a*) keep your eyes steadily upon the fixation-point; (*b*) fixate the surface of the moving field; (*c*) look through and beyond the screen and fixate at 'infinity.' When the objective movement has ceased and the resulting phenomena have run their course, give a complete account of them."

While the *O*'s had their eyes closed the motor was started, and the screen was allowed to reach its full velocity before the 'Now' was given. At the end of the experiment the screen was instantly stopped; and when the after-effects, if any, had run their course, the *O*'s turned to the description.

Every experiment was repeated twice with every exposure time. The number of times, expressed in percentages, that an after-effect was reported, is shown in Table I.

TABLE I

SHOWING THE PER CENT. OF CASES IN WHICH THE AFTER-EFFECT WAS REPORTED WHEN WOHLGEMUTH'S APPARATUS WAS USED

	Observed		
		Without Fixation-Point	
O	With Fixation-Point	Near Fixation	Far Fixation
A	33%	100%	66%
Da	16%	100%	33%
Di	0%	100%	100%
T	66%	100%	100%
W	100%	100%	100%
1	50%	100%	100%
2	50%	33%	100%
3	66%	66%	66%
4	16%	0%	0%
5	0%	0%	0%
6	0%	0%	66%
Average	36.3%	63.6%	66.6%

[11] *Ibid.*

The after-effect was variously reported as a "slow upward movement," a "rapid upward movement," a "reverse movement," a "rebound," a "jerk upward," a "recoil," a "spring up." The direction of movement in the after-image was always opposite to that of the objective stimulus. The rate, extent, and duration of the movement varied considerably: it was described as rapid, as medium, and as slow; it was local (around the fixation point), general (extending over the entire field), or intermediate between these extremes; it passed through a small angle and was reported as "upward about an inch," or through a large angle and was reported as "upward a foot," or it was intermediate; and it was of short, intermediate or long duration.

The apparatus as employed by Wohlgemuth, with a fixation-point, afforded on the whole the least favorable method for observation of the phenomenon. But even under these unfavorable conditions the *O*'s, on the average, reported its presence in more than one-third of the experiments. The individual differences between *O*'s was large. Of the trained *O*'s, W reported the after-effect in every experiment, wheras Di failed to observe it in a single case; of the untrained *O*'s, 1, 2, and 3 reported an after-effect in at least half of the experiments, whereas 4, 5 and 6 reported it in few or none.

The after-effect was noted in a larger percentage of the cases when the fixation-point was removed. The figures given in Table I show that an after-effect was, on the average, reported by all the *O*'s in about two-thirds of the cases.

There is very little difference in the results with near and with far fixation: an after-effect was reported in 63.6% of the experiments with near, and in 66.6% of those with far fixation. In these experiments the individual differences among the trained *O*'s was very small: with near fixation every *O* reported the phenomenon after every experiment; with far fixation Di, T, and W continued to report the after-effect after every experiment, whereas A and Da failed to observe it in ⅓ and ⅔ of the experiments respectively. The untrained *O*'s show greater individual variation. Some gave descriptions which compare with those of the trained *O*'s, while others, like Wohlgemuth's *O*'s, failed to observe an after-effect.

The effect of practice and training is clearly marked. Those *O*'s who had taken the practice series reported the after-effect more frequently and more consistently than the untrained *O*'s. Two of the untrained *O*'s, 1 and 2, gave reports and percentages which resemble those of the practised group. These *O*'s were both psychologists, and their general training in observation sufficed to differentiate them from the unpractised and untrained group.

In a number of the experiments in which no after-effect was observed the *O*'s stated that the objective stimuli fused as in color-mixing; the white and black lines disappeared, and the background became a uniform grey. Fusion was more frequently reported with fixation, when the eyes were strained and attention was concentrated upon the white square, than without.

We conclude from our repetition of Wohlgemuth's experiments (1) that the apparatus is unsuited to the problem. For (*a*) the entire visual field is not covered by an objectively moving stimulus; (*b*) in order to approximate this condition the *O*'s must stand so near the moving field that the black and white tend to fuse; (*c*) fusion is especially noticeable in experiments in which a fixation point, with

the resulting strain of convergence and concentration of attention, is introduced; and (*d*) the presence of this point and of its wire support violate the conditions of complete objective movement; the point of clearest vision and maximal attention is stationary. We find (2) that even under the unfavorable conditions of Wohlgemuth's procedure the after-effect may be observed. The reports vary considerably, but correlate roughly with the training and experience of the *O*'s. Trained *O*'s and experienced *O*'s without special training report a reverse movement in about half of the experiments. We note (3) that if the problem of the after-effect of complete visual movement is to be successfully attacked we must change the apparatus: (*a*) the whole field of vision must be stimulated; (*b*) the fixation point must be eliminated; and (*c*) the distance of *O* from the moving field must be large enough to rule out eye-strain.

Principal Experiment.—The apparatus which we finally constructed consisted of a large cylinder 1.7 m. high and 1.25 m. in diameter.[12] On the inside we glued heavy white architect's paper on which had been drawn in carbon ink at intervals of 7 mm. black lines 7 mm. in width. The cylinder was supported from above by a shaft projecting from a ball-bearing swivel joint.[13] It was rotated by an electric motor at two rates of speed, which we shall hereafter designate as 'fast' and 'slow.' At the fast rate the cylinder moved with a velocity of nearly 60 cm. per sec., at the slow rate with a velocity of approximately 30 cm. per sec.[14] The times of stimulation were 5, 15 and 25 sec.[15] The apparatus was placed in a dark room, so that the illumination was under control. The cylinder was lighted from within by a daylight mazda lamp placed above and slightly in front of the *O*. There were no shadows, and the illumination was uniform over the whole field of vision.

The *O* sat upon a stool at the centre of the cylinder. A biting board, rigidly supported from the floor, was employed to insure a constant distance of 29 cm. between *O*'s eyes and the cylinder wall.

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[12] An apparatus of this form was used by J. Aitken in 1878 (*Proc. Royal Soc. Edin.*, x, 1878, 40 ff.; *Journ. Anat. Physiol.*, xiii, 1879, 322 ff.) with negative results. The cylinder was rotated about the *O* and then lifted; no after-effect was observed upon the surrounding objects. The sole uniform experience was a disagreeable, sickening effect. E. Budde employed the same apparatus in 1884 (*Arch. f. Anat. u. Physiol.*, 127 ff.). The experiences set up were so trying that the experiments were soon broken off.

[13] This is a part of the apparatus for localisation of sounds described by M. Bentley, this JOURNAL, xxiii., 1912, 509.

[14] The exact rates were respectively 57.3 and 34.1 cm. per sec. Cf. Wohlgemuth, *op. cit.*, 28, par. 8

[15] We had found, in our preliminary experiments, that the short exposure times were as effective as the long to produce the after-image of movement. Nausea and dizziness, as the two earlier authors found (footnote 12), result from the stimulation of the cylinder; but they may be eliminated by shortening the time of exposure. With the intervals used, only two *O*'s reported these experiences; and in neither case were they sufficiently strong or unpleasant to force a withdrawal from the experiment. As the experiment progressed these *O*'s became habituated, and (like the other *O*'s) were unaffected even by the 25 sec. stimulation.

This distance, chosen after trial, proved very satisfactory; there was no noticeable eye-strain.

The cylinder was stopped by a brake operated from *E*'s desk by a rigid lever-arm 3 m. in length. The effect was absolute and instantaneous. Great care was taken with this part of the apparatus; and records by the graphic method showed that the arrest of motion was sharp and abrupt; there was no indication either of continued movement or of rebound when the brake was applied.

There was no fixation point. The *O*'s were directed in half of the experiments to fixate the surface of the cylinder and in half to look through and beyond the screen, *i.*, to fixate at 'infinity.'

We had, then, two velocities, a fast and a slow; three durations, 5, 15, and 25 sec.; and two fixations. We had also separate instructions for meaning and for process. The series were repeated 5 times.

The *meaning* instructions were as follows: "When the signal 'Ready' is given close your eyes. The cylinder will then be set in motion. When the proper velocity has been reached the signal 'Now' will be given. On the 'Now' open your eyes and (*a*) fixate the surface of the screen at a point directly in front of you; (*b*) look through and beyond the screen, fixate at 'infinity.'"

In the preliminary experiments you reported, among other things, an after-effect of various degrees of intensity, duration, and extent. You are now to characterize this perception as fully as you can. Designate the degree of movement in the after-image on the following scale:

0. no after-effect
1. after-effect poor
2. after-effect fair
3. after-effect good
4. after-effect very good
5. after-effect excellent

When the objective movement has ceased and the resulting phenomena have run their course, give an account of them."

In the instructions for process the same general *Aufgabe* regarding signals and fixation was laid down. The specific instructions for meaning were omitted, and the following sentences were added: "In the preliminary experiments you reported after-images of movement. In this experiment you are to concentrate upon description. When the objective movement has ceased and the resulting phenomena have run their course, describe the mental processes in strictly psychological terms."

Only the practised *O*'s observed in this part of the experiment. The order in which the series were presented was different for every *O*, as follows:

Series	Observer				
	A	Da	Di	T	W
Meaning, near fixation.....	3	h a p h a z a r d	2	1	4
Meaning, far fixation.....	4		3	2	1
Process, near fixation.....	1		4	3	2
Process, far fixation.....	2		1	4	3

Results.—The results of the experiments upon 'meaning' appear in Tables II and III.

TABLE II

SHOWING THE NUMBER OF CASES IN WHICH THE AFTER-EFFECT WAS REPORTED, DISTRIBUTED ACCORDING TO THE POSITION OF FIXATION, THE RATE OF THE OBJECTIVE MOVEMENT, AND THE DURATION OF THE STIMULATION

O	Fixation	Rate	Duration				Total Near or Far	Total Slow	Total Fast
			5	15	25	Total			
A	Near	S	2	3	4	9	13	20	7
		F	1	1	2	4			
	Far	S	3	4	4	11	14		
		F	1	1	1	3			
	Total		7	9	11	27			
Da	Near	S	5	5	5	15	28	28	27
		F	4	4	5	13			
	Far	S	3	5	5	13	27		
		F	4	5	5	14			
	Total		16	19	20	55			
Di	Near	S	1	1	4	6	12	9	7
		F	1	2	3	6			
	Far	S	0	1	2	3	4		
		F	0	0	1	1			
	Total		2	4	10	16			
T	Near	S	4	5	5	14	27	25	26
		F	3	5	5	13			
	Far	S	2	4	5	11	24		
		F	4	4	5	13			
	Total		13	18	20	51			

O	Fixation	Rate	Duration				Total Near or Far	Total Slow	Total Fast
			5	15	25	Total			
W	Near	S	5	5	5	15	30	30	30
		F	5	5	5	15			
	Far	S	5	5	5	15	30		
		F	5	5	5	15			
	Total		20	20	20	60			
Summary			58	70	81	209	110/99	112	97

Table II summarises the cases in which the after-effect was reported by the *O*'s according to the position of fixation, the velocity of the objective movement, and the duration of the stimulation.

Under our conditions a negative after-image of movement was perceived by all *O*'s. This result was corroborated by those of the unpractised group, which now comprised 12 *O*'s. A series of five experiments, with near fixation, fast velocity, and a 25 sec. exposure, was taken with every member of this group. The meaning instructions alone were used. Four *O*'s reported the perception of reverse movement in every experiment; three in 80%; one in 60%; three in 40%; and one in 20% of the experiments. It should be recalled that all investigators have found that the after-effect becomes more pronounced with practice, and that this group approached the problem without the slightest training.

The results obtained from the two groups of *O*'s in this part of the experiment indicate that an after-effect occurs when the whole visual field is filled by an objective moving stimulus. Compulsory conditions, however, were not obtained for every *O*.

In the practised group compulsory conditions were obtained for three *O*'s: W reported the after-effect in every experiment, no matter how (within our limits) the variables were arranged; T reported it in every case when the stimulation had been extended to 25 sec.; and Da in every case when the fixation was 'near' and the velocity slow;' also in every case, no matter where the fixation or what the velocity (within our limits), when the stimulus was extended to 25 sec.

Compulsory conditions were not obtained for A and Di. Their reports, however, indicate the approach of compulsion: an increase in time of stimulation is paralleled by an increase in reports of the after-effect. For the 5 sec. exposure these *O*'s report the after-effect in 7 and 2 cases respectively; for the 15 sec. exposure, in 9 and 4 cases; and for the 25 sec. exposure, in 11 and 10 cases. Had our variable conditions been slightly extended, or had the time of stimu-

lation alone been lengthened,[16] it is very probable that we should have obtained compulsory conditions for them also. Owing to the short duration of the summer session it was impossible to carry the experiment further.

The summary at the foot of Table II shows that long stimulation, near fixation, and slow movement are, within our limits, the most favorable conditions for the perception of the illusion. As in the data of A and Di, so also in the results of the other *O*'s, the after-effect is most frequently reported when the exposure is extended to 25 sec. The summary also shows that the after-effect is reported 110 times with near fixation, 99 times with far; 112 times with slow rotation of the cylinder, 97 times with fast. The differences here are slight, and probably of little significance, since they are primarily due to the results of a single *O:* to those of Di in the matter of position of fixation, and to those of A in that of rate of rotation. Our conclusion is that position of fixation and rate of objective movement are irrelevant moments, and that the conditioning factor is the duration of stimulation.

These conclusions are confirmed by the *O*'s estimate of the cognitive clearness of the after-effect, which is presented in Table III.

These results parallel those of Table II. The *O*'s who report comparatively few cases of an after-effect place them for the most part at the lower end of the scale, whereas those who report many cases distribute them toward the upper end.

The results regarding the variable moments were likewise confirmed: the distribution upon the clearness scale for 'near' and 'far' fixation, and for 'slow' and 'fast' objective movement, shows no relevant differences, whereas the distribution according to the duration of stimulation shows a shift from the lower end for the short exposures to the high end for the long exposures. Even in the case of W, who reported the phenomenon in every experiment, the clearness of the after-movement shifts with the longest exposures to the highest degrees.

An analysis of the introspective reports yields the following results:

(1) The movement, given in the exposure and in the after-effect, is at times subjectified. Some *O*'s report that they revolved as on a swivel chair, and that the screen was stationary. The after-effect in these cases is merely a reversal, as in the rotary chair experiment, of the direction of the subjective movement. Examples are: (A) "Swimming sensations; I revolved in direction opposite to that of the cylinder. When the objective movement stopped, I rotated in the opposite direction;" (Di) "Pressures in head; feel of moving backward; this feeling kinaesthetic not visual;" (W) "Seemed as if I were doing the moving; felt as if I were moving to the right" (rotation of cylinder was to left).

[16] Wohlgemuth found that short stimulations of 5 sec. or less (Exp. 31, p. 85) failed to produce an after-effect of movement. He used exposure times varying from 20 sec. to 3 min. The majority of his experiments were performed with times lying between 30 and 60 sec. Our preliminary experiments showed that it was possible to obtain the illusion with the shorter times. The obvious advantage of using these shorter exposures lies in the fact, mentioned above, that the disagreeable and distracting effects, nausea and dizziness, caused by the rotating cylinder, were reduced to a minimum.

TABLE III

SHOWING THE NUMBER OF CASES IN WHICH THE AFTER-EFFECT WAS REPORTED AT THE VARIOUS LEVELS OF COGNITIVE CLEARNESS, AND A DISTRIBUTION OF THE CASES ACCORDING TO THE POSITION OF FIXATION, THE RATE OF MOVEMENT AND THE DURATION OF THE STIMULATION

O	Degree	Total No. Cases	Fixation		Rate		Duration		
			Near	Far	S	F	5	10	15
A	0	33	17	16	10	23	13	11	9
	1	10	5	5	8	2	3	4	3
	2	8	3	5	7	1	2	3	3
	3	5	3	2	4	1	2	1	2
	4	3	2	1	1	2	0	1	2
	5	1	0	1	0	1	0	0	1
Da	0	5	2	3	2	3	4	1	0
	1	23	15	9	13	11	11	7	6
	2	11	4	7	3	8	3	3	5
	3	12	5	7	8	4	1	8	3
	4	6	3	3	3	3	1	1	4
	5	2	1	1	1	1	0	0	2
Di	0	44	18	26	21	23	18	16	10
	1	13	9	4	9	4	2	2	9
	2	0	0	0	0	0	0	0	0
	3	2	2	0	0	2	0	1	1
	4	1	1	0	0	1	0	1	0
	5	0	0	0	0	0	0	0	0
T	0	9	3	6	5	4	7	2	0
	1	29	13	16	19	10	9	11	9
	2	19	13	6	6	13	3	7	9
	3	3	1	2	0	3	1	0	2
	4	0	0	0	0	0	0	0	0
	5	0	0	0	0	0	0	0	0
W	0	0	0	0	0	0	0	0	0
	1	3	2	1	2	1	3	0	0
	2	11	7	4	8	3	7	3	1
	3	3	1	2	3	0	1	1	1
	4	18	9	9	9	9	8	8	2
	5	25	11	14	8	17	1	8	16

(2) The black lines are at times seen in perspective; they are objectified, and appear as rods set at various distances from the eyes. All the *O*'s report experiences of this kind. Examples are: (A) "The lines stood out in perspective. Three in a group; that is, three were equally near, then the next three were back, next three close, and so on;" (Da) "At times the lines were seen in perspective, like black rods arranged at various distances in front of the observer;" (Di) "Lines stood in perspective;" (T) "The projection-effect was very

conspicuous; every second line seemed to stand out;" (W) "Once during the experiment there was a projection of the lines." "Twice during this experiment I noticed a projection; lines stood out in model-effect on grey background."

(3) The lines at times lose their shape and outline. Patterns of varying degrees of complexity appear upon the ruled surface. Simple changes (the lines become fringed and nodulated) and complex changes (large diamond and zig-zag effects) are of about equally frequent occurrence. The appearance of the simple and complex patterns can be correlated roughly with the slow and fast velocities. Examples are: (A) "The lines during motion became small squares, with vertical and horizontal cross-lines. These patterns appeared first at the center of the field of vision, and radiated until the entire field was covered;" (Da) "During the objective movement the field became spotted; the lines lost their regular outline;" "lines became nodulated;" (T) "Grey fringe to the right edge of the lines;" (W) "Lines became ripply;" "lines interwoven into complex pattern."

(4) Color effects are produced by the colorless stimuli. The hues are brilliant and filmy. They flicker and fluctuate. Sometimes they change as in the 'flight of colors;' sometimes antagonistic colors alternate; sometimes two colors (always antagonistic) appear together. Thus: (A) "The lines during motion became brilliantly colored;" (Da) "Left edge of lines blue, right edge yellow;" "lines alternately tinged with yellow and blue;" (T) "During objective movement the lines became wavy. The left side was colored, quality changing from yellow to red, to blue, to green, etc. The wavy figures instable, rapidly changing form and color;" (W) "During motion the lines became tinged with blue."

(5) The lines fuse as in color-mixing into a uniform background, in which case there is no after-effect; or into a flickering background as when the rotation of the color-mixer is too slow, in which case the after-effect is either not observed or if observed is reported as of low degree. Thus: (A) "Lines blended; there was no after-effect;" (Da) "Lines were nodulated; seemed to be stationary blinking spots" (no after-effect of movement was reported); (Di) "Faded to a shimmering gray." Di frequently reported this phenomenon; (T) "Lines became thicker in width at some points and thinner at others. Positions of thinness and thickness not constant, continually changing;" (W) "Toward end the whole thing became a mottled grey" (the after-image of movement was reported as poor).

(6) As Aitken and Budde discovered, stimulation from within the cylinder produces a widespread organic and muscular response. Dizziness and nausea are frequently reported, and tend to obscure the process-configuration. Examples are: (A) "Swimming in head;" (Da) "Strain sensation in muscles of eyes and pressure within head. Dizziness and an organic stir-up;" (Di) "Intense pressure in head, eye strain, and slight dizziness;" (T) "Very strong strain in eye muscles and dull pressure in head;" (W) "Very dizzy and nauseated, pressure sensations in head."

The occurrence of the after-effect can be directly correlated with the arousal of these phenomena. Di, who reports the after-effect least often, reports dizziness but once; W, who reports the after-effect in every experiment, reports dizziness most frequently. This correla-

tion does not indicate, however, that 'dizziness' is essential to the appearance of the after-effect. No *O* reports dizziness every time that he reports the after-movement. It is only in the early experiments that the correlation, even with W, invariably occurs. It appears, therefore, that the optimal conditions for the after-effect are also optimal for the perception of dizziness. It is not until these perceptions became habituated that the process-configuration becomes clear.

(7) The after-effect is reported in precisely the same terms as in our previous experiments. The same perception, an after-image of movement opposite in direction to that of the stimulus, and differing only in extent, is obtained when the entire visual field is filled by an objective moving stimulus as when, with James' waterfall apparatus, the stimulus is limited to a small area of the retina.

(8) The introspections at first include kinaesthetic and organic complexes. As the experiments progress and the *O*'s become accustomed to the experiences, the inessential elements drop away, leaving a residue of pressure and visual sensations.

The pressure sensations within the head never entirely disappear. They are always present when the after-effect is reported.

The visual configuration reported by three of the *O*'s—A and Di, even in the experiments under process-instruction, do not get beyond statements of meaning—is a qualitative and temporal, and perhaps spatial, integration. Examples are: (Da) "Immediately after the objective movement ceased the field became momentarily a blur and grayish; then the vertical lines stood out distinctly again;" "The gray blur was of short duration. On the side of meaning movement rapid;" "When objective movement ceased, field seemed to be covered by a gray cloud; this lasted for a short time; then the vertical lines appeared, distinct in outline. On the side of meaning movement slow;" "When objective movement ceased there was a momentary gray blur, which was followed by distinct vision. On side of meaning a rebound;" (T) "When the objective movement stopped there was a grey; the lines became wider; and then they became distinct again;" "When the objective movement stopped I saw gray like a film;" "When the objective movement stopped the lines momentarily appeared clear in outline, then expanded to right. Over these lines were gray lines; these were indefinite and instable" (reported movement as of the 2nd degree); (W) "When movement ceased lines blurred into a gray;" "After objective movement ceased each line seemed to become broader; meaning, movement to right;" "When objective movement ceased a gray screen appeared; this seemed to move to right."

Summary

(1) We divided our *O*'s into two groups; the one group was given a practice series before the main experiments were undertaken, the other group approached the problem naïvely.

(2) Repetition of Wohlgemuth's work showed:

(*a*) that his apparatus is unsuited to the problem;

(*b*) that even under unfavorable conditions the after-effect is reported in a large percentage of cases by *O*'s of both groups. Individual differences are large. and the effect of practice and training is clearly evident;

(*c*) that the apparatus, if the problem of the after-effect of complete visual movement is to be successfully attacked, must be variously modified.

(3) Experiments with a large cylinder, constructed to meet the requirements of complete visual stimulation, showed:

(*a*) that the after-effect is observed when the whole visual field is filled by an objective moving stimulus;

(*b*) that position of fixation and rate of objective movement (within our limits) are irrelevant moments, and that the conditioning factor is the duration of stimulation;

(*c*) that the characteristic processes of the after-effect are pressure sensations within the head and a qualitative, temporal, and perhaps spatial integration of visual sensations.

(4) Wohlgemuth's failure to attempt a reconciliation of his theoretical views with the negative results of his Exp. 22 turns out to be of no consequence: not because the results of that experiment are unimportant, but because under strict conditions they are positive. As it happens, Wohlgemuth's two negatives amount to an affirmative.

BOOK REVIEWS

The religious consciousness: a psychological study. By J. B. Pratt. New York, The Macmillan Co. 1920. pp. viii, 486.

This very substantial and comprehensive addition to the growing literature of the subject furnishes new evidence of the pre-eminence of American scholars in the psychology of religion. The reader may find here in very clear and attractive form the results of this twenty-one-year old science. The chapters arrange themselves into four groups. The first four may be considered as introductory, dealing with the definition of religion, the nature of the psychology of religion, the significance of the subconscious and the relation of society and the individual. The next five chapters cover the topics and materials presented in several books beginning with Starbuck's. The discussions treat of the religion of childhood, adolescence, conversion and revivals. Six chapters follow which contain perhaps the most original and vital pages of the entire book. The subjects are belief in God and immortality, the nature of cults, and the phenomena of worship and prayer. The last five chapters give an exposition of mysticism and a criticism and estimate of its central principles and practices. Certain problems in each of these groups of chapters will be considered in this review.

The preface emphasizes the purpose of the author to give a description of the religious consciousness in a purely objective way without the bias of any point of view. This he succeeds in doing unless it is in certain passages touching upon the work of some writers whose point of view is avowed and different. The definition of religion given is suggestive of the procedure. "Religion is the serious and social attitude of individuals or communities toward the power or powers which they conceive as having ultimate control over their interests and destinies." The attractiveness of this definition lies in the use of the term "attitude" instead of belief, and in the recognition, though somewhat faint-heartedly, of the social character of religion. The difficulty which the author has with this term 'social' gives the impression that it is forced upon his attention by the drift of current thinking, but without his full consent or satisfaction. He still regards the social as in some way opposed to the individual, but the relation is not clearly set forth. For example, he would seek the origin of religion "within the subjective needs of human nature," but "the religious consciousness inevitably considers its religion objective as well as subjective." Subjective here apparently means subjective to the individual, and objective means more objective than writers like Durkheim would imply by the word social. By thinking of the social as subjective the author fails to recognize the force which the expression "social consciousness" has for those writers whose use of it he criticizes. In connection with the definition it is indicated that the "power or powers" referred to may be regarded as equivalent to the "Determiner of Destiny." The latter expression is the more frequently used. The value of this term evidently lies partly in its vagueness. It is not intended to designate exclusively a personal object of the religious attitude, although it might comprehend that with the

non-personal types. "To describe the workings of the human mind so far as these are influenced by its attitude toward the Determiner of Destiny, is the task of the psychology of religion."

The second group of chapters gives an excellent restatement of the development of religion through childhood and adolescence. The author holds with practically all present-day psychologists that there is no one religious instinct. He rejects the conception of childhood which holds that "Heaven lies about us in our infancy!" He says "*Earth* lies about us in our infancy," and the baby is a little animal. Family influence is the most potent in religious development, but all social contacts make their contribution. Adolescence is "the flowering time for religion." It is, however, full of ups and downs, of storm and stress. The individual gains in power and selfhood but at the same time experiences bewilderment and depression. The essential thing about conversion is the unification of character, the achievement of a new self. Many conversion-experiences are quoted from various religions and sects. Revivalism is criticized adversely for the most part, with an analysis of its methods of emotional, mob suggestion.

The third group of chapters introduces more new observations and discussions than any other part of the book. The two main questions raised concerning God are: why do people continue to believe in God? and what are the psychological factors that influence or determine the meaning of that term? It is observed that the idea of God is generally not very clear. A good deacon is quoted as confessing that his idea of God was "a kind of oblong blur." Sociological influences, such as the form of family and political control, have much to do with impressing the fatherly and kingly patterns upon the idea. The psychological influences are largely in the field of sense and imagination. Images, pictures, lights and sounds are conspicuous. Both sensuous and conceptual factors belong to the idea of God, though the vividness and definiteness of these factors vary greatly in individuals and in the different moods of the same person. There is found here a certain justification for the anthronomorphic image of God which so many persons have struggled to eliminate. The image may be merely symbolic, but it has great power over the emotions. The imaginative and the conceptual aspects of the idea of God, as of other ideas, are not necessarily in conflict but are normally, though with varying emphasis, present and vital. The reasons why people believe in God are put under four types, authoritative, reasoned, emotional and volitional. These types are illustrated by instances received through questionaries. Under the reasoned type appear the influences of the traditional arguments, the cosmological and the teleological. Others reason from the practical effects for good upon the lives of those who believe. The emotional type seems to be more numerous than any of the others. This includes cases of the "experience of God's presence," as among the mystics. The volitional type embraces those whose faith arises from a more or less deliberate "will to believe." The same four types of belief are cited with reference to immortality. Belief in immortality is regarded as coming earlier and as being less dependent upon instruction than belief in God. It is more a matter of feeling and will, of instinctive impulse toward self-preservation. There are, however, many people who do not care for immortality, and there seems to be a lessening desire for it through the last five hundred years because, partly at least, this world has become more attractive.

A good account is given of the cult and its function. The author's travels in India and his observation of the ceremonies of the Roman Catholic church have enabled him to appreciate the poverty and barrenness of Protestant services so far as symbolism is concerned. Accepting the genetic account of the writers who find the origin of the cult in social activities, Professor Pratt seems to fail to recognize the way in which the sense of the group may also supply the equivalent of the "Determiner of Destiny," and how such writers as Durkheim, Jane Harrison, and Cornford have shown the dependence of the cosmic upon the social consciousness. The latter point is dismissed too easily (p. 261). The use of images and idols in religious ceremonies comes out in a new light when viewed in reference to the value of these as means of enlivening religious faith and feeling. "The sensuous representation reinforces the reality-feeling. Much more may thus be said in defence of the practice of 'idolatry' than most of us have been brought up to suppose." Further, the activities of the cult have a similar value for the participant. "The feeling of these acts is a considerable part of the religious emotion." To be most potent in one's experience it must have been cultivated in childhood. An interesting comparison is made between the objective worship of the Roman Catholic church and the subjective worship of Protestantism. The Catholic church occupies itself with the worship of God. The Protestant seeks the salvation of souls. It is immaterial to the former whether a congregation is present, but Protestantism is dependent upon the presence of people. The candles of the Catholic churches are for God; the flowers of the Protestants are for the people to see. The author elaborates this contrast very convincingly. It is doubtful, however, whether he has not overstressed the subjective phase of Protestant services. Does any one "attempt to produce merely subjective religious effects?" Here an effect achieved with the group seems to be considered as much subjective as the emotional state of an individual. But it often happens that the common will of an assembly deliberating upon measures of public welfare communicates itself to the individuals as an objective, super-individual judgment. It does not seem quite exact to designate that consensus of opinion and feeling as "subjective." The author admits that "objective worship of the sort that aims to please the Deity is a thing of the past" (p. 308). His description of objective worship other than this is not quite clear. He surely does not mean that men can worship "cosmic forces." It is likely that the author has not yet spoken to his own satisfaction on this point. Having so qualified his "Determiner of Destiny" as to render it unable to take the rôle of God as commonly conceived, he does not adequately provide for the central factor in religious worship. The same problem concerning the meaning of the word subjective affects the discussion of prayer. "If it be true both that the subjective value of prayer is very great and also that it is the only value which prayer possesses, this latter fact should be assiduously kept secret."

Professor Pratt gives more space to mysticism than is found in any other general treatise on the psychology of religion. Mysticism is defined "as the sense of the presence of a being or reality through other means than the ordinary perceptive processes or the reason." Two types are distinguished, the mild and the extreme. Extensive quotations are cited to set forth their characteristics. The sense of presence is the largest factor. This is often most vivid when the mystic

is alone on the hills or in the woods. It is susceptible of cultivation; and the literature of mysticism is much occupied with the technique and discipline by which its goal is reached. Pratt recognizes the fact that many mystics are abnormal and that their quest for this great experience has brought the illusion of attaining it. Still he does not share the views of Murisier and Leuba in this respect, but holds that mysticism has positive value and adds assurance and vividness beyond ordinary religious experience. In the discussion of mysticism, as in other parts of the book, the author shows his close affinity to William James.

The style is clear and entertaining. The use of first-hand material secured by questionaries adds concreteness and vividness, but there can be little scientific value in trying to employ the method of percentages on 170 replies concerning prayer or belief in God! The abundant references and extensive footnotes open into attractive elaborations of the discussions in the text. While scarcely prepared in the manner of a text-book, the book will undoubtedly be used for that purpose and as collateral reading. For the general reader and for the teacher and preacher of religion it affords one of the most adequate treatments of the subject to be had. It is critical and discriminating, but it is also decidedly friendly and constructive.

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Das jüdische Volk und seine Jugend. By S. BERNFELD. Wien, R. Löwit Verlag, 1920. 149 pp.

The first half of this book is devoted to a negative criticism of the aims and results of European education. Since the child is looked upon as an adult 'in the rough,' whom it is the function of the educational process to polish into adulthood, the results of European education are bound to be an unnatural forcing of the child's development and the destruction of the cultural possibilities inherent in youth.

The second half of the book gives a vivid presentation of a system of education to prevail half a century hence in Palestine, which is conceived as possessing by that time the seat of a Jewish commonwealth. The national constitution provides for the complete support and control by the state of the entire youth of the country from birth to the age of twenty. The young people are concentrated in settlements in different parts of the country, under the guidance of a psychologically and medically trained corps of educators, whose ffice is merely to observe the development of their charges and to be ready to aid them in their spontaneous efforts to acquire any particular discipline or skill. Within these communities the great thinkers and artists of the country, pursuing their work in a congenial atmosphere and drawing interested groups around them, create centers for cultural growth.

This Utopia can only be realized in a country whose productivity, necessarily high, accrues not to the individual but to the state, and among a people who appreciate youth for its own sake,—therefore in a Jewish Palestine. S. FELDMAN

NOTES

A NOTE ON "VOCALITY"

In a recently published paper,[1] Dr. Weiss attempts to redefine the 'attribute' of vocality. He seeks to identify judgments of vowel-character with that attribute which is describable in terms of mellowness and shrillness, and asks all investigators to use the name "vocality" for this characteristic.[2] It would seem advisable to examine critically the foundations upon which such a classification may be based.

A psychophysical study of the tonal attributes brings forth evidence, cumulative rather than direct, that judgments of vowel-quality are not made upon a purely attributive basis, but result from the observer's experience in the use of spoken language.[3] This Dr. Weiss tacitly admits when he says that "when a subject is asked to discriminate the vowel character of a fork tone, the reaction is one that is already well established in the ordinary (speech) habits of the individual."[4] But he regards these habits as merely selecting the conventionalized points in an already existent continuum, which may be described either in vocal terms or in terms of mellowness and shrillness. Moreover, these conventionalized points occur in an octave relationship, because the process of conventionalization picks, as the vowels of any language, combinations of mellowness and shrillness that are separated by equal sensorial steps, corresponding with logarithmic intervals in the series of physical frequencies. The octave-relationship is, then, to be regarded as manifestation of the Weber-Fechner Law.[5]

My own study of the tonal attributes shows that the limen for vocal judgments does not follow Weber's Law.[6] Of the tonal attributes, volume is the only one that gives indication of following this law.[7] A comparison of the differential limens for volume and for vocality shows that the former are uniformly the larger of the two, even though the vocal judgment is more difficult and less immediate than is the judgment of volume. This relationship, of course, precludes the possibility that the vowels are judged upon a basis of volume, that is to say, upon the basis of the characteristic of pure tones that obeys the logarithmic law.

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[1] A. P. Weiss, The Vowel Character of Fork Tones, *Amer. Jour. of Psychol.*, 31, 1920, 166 ff.

[2] *Op. cit.*, 191.

[3] G. J. Rich, A Study of Tonal Attributes, *Amer. Jour. of Psychol.*, 30, 1919, 131 ff. Although Dr. Weiss' paper appeared later than mine, it would seem that he wrote it at an earlier date.

[4] *Op. cit.*, 192 f.

[5] *Op. cit.*, 190 f.

[6] G. J. Rich, *op. cit.*, 139.

[7] *Op. cit.*, 149 ff. Also A Preliminary Study of Tonal Volume, *Jour. of Exper. Psychol.*, 1, 1916, 13 ff.

When he invokes the Weber-Fechner Law as an explanatory principle,[8] Dr. Weiss narrows his conception of vocality to that of a characteristic which shows the octave-relationship found only by Köhler. The trend of recent experimental work tends to show that the vowels of a language (Dr. Weiss' 'conventionalized vocality combinations') seldom occur an octave apart.[9] Köhler's experimental situation was such that, as Stumpf has suggested,[10] his observers may well have tended to judge as the 'pure vowels' the same tonality occurring in successive octaves. This would seem to be a sufficient explanation of the relationship found by Köhler, especially in view of the subsequent lack of verification.

On the other hand, vowel-character is considered by Dr. Weiss as a continuum which is to be described in terms of mellowness and shrillness, terms which are descriptive of pitch-brightness. In so far as the limens for pitch-brightness are in all cases lower than the vocal limens, this attribute may serve as the basis for the perceptual judgments of vowel-likeness.[11] The successive character of the qualities that form the pitch-brightness series is definitely in line with what is seemingly the only constant factor in the results of the many investigations of vowels, the order in which the vowels follow one another as one ascends the musical scale.[12] But the possibility that vocal judgments are based upon pitch-brightness does not necessarily *identify* whole vowel-series within a range of little over an octave. *Op. cit.*, 133. them with the latter attribute. The very fact that the limens for vowel-character and for pitch-brightness are vastly different, both in magnitude and in course,[13] is evidence that the vocal judgment is not made singly and unequivocally upon a basis of pitch-brightness, that is to say, it is not merely a judgment of pitch-brightness in a 'conventionalized combination.' There is not at present sufficient evidence to warrant the assertion that the vocal perception (or 'habit') is a single attributive continuum describable in non-vocal terms, in terms of one or more of the attributes of pure tones.

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[8] The Weber-Fechner Law would explain fully as satisfactorily the occurrence of the pure vowels (the 'conventionalized vocality combinations') at any intervals other than the octave, *e.g.*, at intervals of fifths, thirds, etc.

[9] D. C. Miller, *The Science of Musical Sounds*, 1916. H. Schole, Über die Zusammensetzung der Vokale U, O, A, *Arch. f. d. ges. Psychol.*, 13, 1918, 12 ff. C. Stumpf, Die Struktur der Vokale, *Sitzungsber. d. Preus. Akad. d. Wissensch.*, 1918, I Halbband, 333 ff. G. J. Rich, A study of Tonal Attributes, *Amer. Jour. of Psychol.*, 30, 1919, 131 ff. Dr. Weiss' own experimental results fail to show an octave relationship.

[10] C. Stumpf, Über neuere Untersuchungen zur Tonlehre, *Ber. ü. d. 6 Kong. f. exper. Psychol.*, 1914, 305 ff.

[11] G. J. Rich, *op. cit.*, 138 f; 144 f; 154 ff; 164.

[12] Thus, unpractised observers may report, in proper succession, the

[13] See Footnote 11.

CONSCIOUSNESS IN THE SIAMESE TWINS

Bolton's account of the Siamese twins in 1830 (G. B. Bolton, *Philos. Trans.*, 1830, 177-186) suggests an overlapping of consciousness which is not unlike the overlapping of two hysterically separated consciousnesses. It will be recalled that the twins, Chang and Eng, were possessed of entirely distinct personalities and sensory equipment; only when the band uniting them was punctured by a needle at its very middle did both shrink, whereas half-an-inch to either side the prick brought response in only the one or the other. Nevertheless Bolton thought he found some community,—an overlapping "unconscious" and a distinct "conscious" perhaps he would have said to-day! He wrote:

"They always fall asleep at the same moment, and it is impossible to wake one without also arousing the other. When they were at Boston, Doctor Skey, Surgeon General to the British Army, entered their bed-room at midnight on three successive nights when both were asleep: on each occasion he touched one and was answered by the other, both awaking at the same instant, inquiring why they were disturbed.

"The experiment has also been repeated in this country, and with the same result. On my tickling one of them, the other told me to desist, though he stated that he did not feel the touch, and it was quite clear that he could not see me tickle his brother.

"On their voyage to England one of them had the tooth-ache, during three days and nights, and suffered great pain, with loss of sleep, which last evil was shared by his brother, both remaining awake. On the 16th of December Mr. Hale went into their bed-room when they were asleep. Eng was restless and tossing about in bed, while Chang was screaming. He awoke them, and on inquiring what ailed them, Eng replied that he was dreaming about his mother, and Chang said that a man was cutting off the long hair from his head. These different dreams appear to have occurred simultaneously" (p. 184).

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